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TOPOGRAPHY ON THE SURVEY OF THE MEXICO-UNITED STATES BOUNDARY.

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WITH DISCUSSION.

The adaptation of plans to the purpose in view constitutes the aim of the civil engineer. However general in scope a method may be, it is modified in characteristic details by ever-varying requirements of natural conditions, of the project, and of the necessary cost. The appreciation of the conditions involved characterizes the successful project, whether in the survey, design or construction of engineering work.

It is for the purpose of showing the adaptability (both in their usual form and in their elasticity to suit various conditions) of well-known surveying methods, and especially of the use of the stadia, that this paper is written. In discussing the survey there will be given a general review of the methods adopted under the various conditions, passing lightly over such details as are of general knowledge and use

at the present day and amplifying more those methods and details which are, perhaps, somewhat new to engineering literature and to surveys as at present conducted.

This boundary survey was made under the direction of two commissions, one representing Mexico, and the other the United States. Each commission made a complete survey, with independent parties, for its own nation. The commissioners for the United States were Lieut.-Col. J. W. Barlow and First Lieut. D. D. Gaillard of the Corps of Engineers, U. S. A., and A. T. Mosman, M. Am. Soc. C. E., of the United States Coast and Geodetic Survey. The field work of the survey was begun in February, 1892, and was finished in October, 1893. During this time the author had charge of the topography under the United States Commissioners.

The survey included all the frontier between the two republics, except the river boundaries, thus excluding that of the Rio Grande from El Paso, Tex., to the Gulf of Mexico and that of about 24 miles along the Colorado River below Yuma, Ariz. The overland boundary as surveyed consists of five segments, as provided in the Gadsden treaty of 1853, starting at the intersection of the Rio Grande and the parallel 31° 47′ north, and running west along this parallel 100 miles; thence due south to parallel 31° 20′ north; thence west along this parallel to the 111th meridian west of Greenwich; thence in a straight line to the Colorado River at a point 20 miles below the mouth of the Gila River; and (the fifth segment) from the point where the Gila River enters the Colorado River in a straight line to a point on the Pacific Ocean 1 marine league south of the south end of San Diego Bay. The last two segments were, then, on neither parallels nor meridians.

This boundary was marked soon after its establishment with as great care as was practicable at that time and in a desert country far from communication and supplies. Fifty-three more or less permanent monuments were erected in the distance of about 675 miles, but many became scattered and lost, so that there were eleven gaps between monuments of about 20 miles and more, and one space of over 100 miles. In consequence there were disputes concerning ownership of property and customs difficulties, and after the lapse of nearly 40 years a second treaty was formed between the two republics, providing for a resurvey based upon the previous one (making authenticated monuments of that survey fixed points of the boundary to be marked again), and provid-

ing that distances between monuments should never exceed 5 miles. In pursuance of this treaty and as determined by the resurvey, permanent monuments of cast-iron have been erected and the old authenticated monuments have been rebuilt of dressed stone in such a way that throughout the boundary the 258 monuments are visible one from another.

The nature of the country to be traversed had a most important bearing upon the organization of the parties and the methods adopted for the survey. Little was known of extensive portions of the boundary, and there was much difficulty in securing definite information concerning it. The fact that about as accurate a map of this region as had ever been made was published nearly two centuries ago (1701) by Father Kino, a Jesuit priest, will indicate the paucity of accurate information. Advantage was taken of the reports on Major Emory's survey (1849–1855), of a reconnaissance by Captain Symons about 10 years ago, and of such other evidence as could be secured. General plans were thus formed, and were modified as found necessary by reconnaissances varying from 50 to 150 miles in advance of the working parties.

The whole region is essentially arid, some portions more so than others, but all natural features and conditions are such as arise from its arid character. The country consists of a succession of plains and mountain chains, the latter extending generally from northwest to southeast and varying in width from 2 to 20 miles. The plains are flat, sandy wastes, rarely showing any irregularity or erosion; the cactus and sage brush form their distinctive and often only vegetation. The mountains vary in height from a few hundred to 4 000 ft. above the plains and are generally igneous or metamorphic in composition. Volcanic peaks are frequent, and at times the lava has poured in a great flood over the plains, covering them for miles with a sheet of liquid fire from a few inches to several feet in depth, and making a desolate region even more desolate and forbidding and furnace-like, as the lava rock catches and throws out again the fierce heat of an unclouded sun. Mountain vegetation here is also arid in character, the cactus being the prominent feature. The hardy and stunted ironwood, manzanita and evergreen oak are found on different ranges, and, very rarely, pine, sycamore and other trees, wherever they can find enough moisture.

The extremely precipitous character of the mountains is remarkable. Vertical angles of 40°, 50° and 60° were often necessary, and cliffs were encountered that could not be scaled; at times it was necessary to occupy with transits pinnacles hardly large enough to admit of the extended tripod legs. As a general illustration, at one point of the boundary a sight of about 2 000 ft. was taken from one mountain ridge to the next, between which there was a ravine about 1 000 ft. in depth. Before 10 o'clock the transitman began his descent from the ridge to cross the ravine and to climb to the point just observed, while he signaled the stadiaman to cross from that point a second ravine to a third ridge for obtaining an advance sight. Approaching sunset forced them both to return to camp before they could reach the intended points. The larger part of the day was taken in the descent and partial ascent, which suggests the rugged steepness of the mountains and the great care necessary to escape disaster. Similar conditions were frequently encountered. The greatest difficulty in this arid country is to secure water. Springs, wells, lagoons and streams were made use of as found, and twice great natural rock basins in the mountains, which caught rain-water and held it long after the rainy season, gave a very opportune supply. Still it was frequently necessary to draw the water 15 or 20 miles to camps, sometimes 50 miles, and in one case over 100 miles. The lack of water and its poor quality often caused inconvenience, but never suffering from its failure, though sometimes the margin of safety was small.

The summer temperatures are very severe; not so trying, however, in the plateau region of the eastern half as in the low-lying desert region on both sides of the Colorado River. Though from design the worst portions of the deserts were passed in the spring, during June and July of the second summer the parties that crossed the Yuma Desert were subjected to a temperature rarely falling as low as 90° through the night, and reaching at least 104° for four or five hours nearly every day, and on one day indicating 118° in the shade. The standardized thermometer would read only 154°, so that the temperature in the sun could not be determined.

The dryness of the atmosphere tempered the great heat in its effects, else it would have been unendurable. The only shade was furnished by the tents, vegetation affording none. Not only were the engineers working constantly in the sun, but, unless within reach of the camp, the noonday rest found no cooling shade, and proved, rather, a hardship.

Temperature, dry atmosphere and rugged mountain ascents all conspired to tax endurance and to necessitate a bountiful supply of water. Men would drink 2 galls. or more a day, suffering no ill effects. The country was, in the main, uninhabited. The Southern Pacific Railroad was generally the base of supplies. This necessitated a haulage, for varying distances up to 80 and 100 miles, of provisions for the engineers and the necessary escort of troops, and of forage for the animals.

Such, then, were the principal obstacles and hardships. They necessitated as small parties as possible, and efficient organization and methods.

The determination of each of the three right-line sections of the boundary, the meridian portion and the two western sections, consisted in the prolongation of the line from its determined direction at one end. The tracing of the two curved-line sections involved frequent astronomical stations. These were established at successive distances of about 20 miles. Observations were made to determine latitude and azimuth, and tangents to the parallel at each astronomical station were laid out and prolonged to the next station. Intermediate points on the boundary were determined by computed offsets from the tangent. A description of methods used for this work does not properly come within the scope of this paper. Descriptions of the latitude and azimuth determinations and of methods used in tracing long straight lines in the field are fully given in the *Transactions* of the Association of Engineers of Cornell University, 1894, by Mr. John F. Hayford, who had charge of this work on the survey.* These tangents and right-line portions of the boundary, as astronomically determined and prolonged, formed the basis for the topographical survey, and the work of the topographical parties begins here.

The whole boundary was measured by the stadia. At the first it was questioned whether the chain or stadia would give the more accurate results, although there was no doubt that in hilly and mountainous regions the stadia would be more accurate and very much more expeditious. Accordingly both were tried together in a country particularly favorable to the chain because of its generally flat and treeless character. Over a portion of the same line, Señors Molina and Contreras determined distances by a well-planned triangulation for the Mexican government in 1855. Corresponding distances as found by stadia and chain are given in the table, distances to follow being always in meters, the unit used also on this last survey.

* E. L. Ingram, Assoc. M. Am. Soc. C. E., had charge of about two thirds of this "line" work of the survey

TABLE OF DISTANCES AS DETERMINED BY STADIA, TRIANGULATION AND CHAIN.

Error.	Stadia.	Triangulation.	Corrected chain.	Error.
$-\frac{1}{800}$	13 171	13 193	13 193	0
$+\frac{1}{18000}$	19 970	19 969	19 965	$-\frac{1}{4992}$
$-\frac{1}{706}$	21 473	21 500	21 474	$-\frac{1}{827}$
$-\frac{1}{808}$	21 598	21 629	21 605	$-\frac{1}{901}$
$-\frac{1}{2803}$	22 813	22 819	22 804	$-\frac{1}{1521}$
$+\frac{1}{635}$	49 957	49 879		
$-\frac{1}{21284}$	148 982	148 989	= Total distances.	

The total ratio of error between triangulation and chain is $-\frac{1}{1430}$ from the table; between triangulation and stadia for the same distance, $-\frac{1}{1160}$; and for the total continuous distance, $-\frac{1}{21284}$.

It will be noticed that over the chain-measured distances the heading is "corrected chain"; this is so stated because at different times the stadia detected the dropping or addition of a chain length in the chaining record. This occurred six times in the above distance, and, when detected, the section in question was remeasured. The error was invariably found to be by the chain and its record thus corrected, otherwise there would have been six errors of chain measurement of 20 m. each. The stadia places all the responsibility for correct measurement on the trained transitman, and so reduces the danger of systematic errors to a minimum. These considerations indicated the greater reliability of the stadia measurement, even in the level country.

During the progress of the survey here described, some other sections were triangulated. All distances so determined are compared with the corresponding stadia distances in the following table:

TABLE OF DISTANCES AS DETERMINED BY TRIANGULATION AND STADIA.

Triangulation.	Stadia.	Error.
223.6	222.7	$-\frac{1}{248}$
411.7	412.7	$+\frac{1}{412}$
757.6	755.0	$-\frac{1}{291}$
785.0	785.3	$+\frac{1}{2617}$
943.8	945.5	$+\frac{1}{555}$
983.3	985.2	$+\frac{1}{518}$
1 273.2	1 272.8	$-\frac{1}{3183}$
1 346.6	1 349.0	$+\frac{1}{561}$
1 788.0	1 789.3	$+\frac{1}{1378}$
4 433.0	4 446.0	$+\frac{1}{341}$
73 095.5	73 056.6	$-\frac{1}{1879}$
86 041.3	86 020.1	$-\frac{1}{4058}$

In considering this table it should be borne in mind that the first ten distances were measured under conditions as severe as are ever likely to occur in surface measurement. The region was a succession of ridges and deep cañons often on too large a scale to admit of sighting from one ridge to the next, and so necessitating large vertical angles; and at times the vertical angle for a reasonable sight would be so great that readings of excessive length would be taken in preference. Its rough, abnormal character may be seen from the fact that it would certainly have taken two or three weeks to chain the distance of about 8 miles, and even then the result would have been very questionable, owing to the error and confusion from incessant "breaking chain." The measurement by stadia occupied about 20 hours.

A third portion of the boundary has been triangulated since the completion of the regular survey. This was done because here the Mexican and American stadia distances showed a considerable disagreement, and so it is reasonable to suppose that it furnishes a check on the poorest, and not the average, stadia work. The comparison of distances with the original stadia measurement is here given by sections:

TABLE OF DISTANCES AS DETERMINED BY TRIANGULATION AND STADIA.

Triangulation.	Stadia.	Error.
257	256	$-\frac{1}{257}$
1 528	1 530	$+\frac{1}{764}$
2 471	2 475	$+\frac{1}{618}$
3 074	3 075	$+\frac{1}{3074}$
3 308	3 310	$+\frac{1}{1654}$
3 324	3 333	$+\frac{1}{369}$
3 334	3 340	$+\frac{1}{556}$
3 475	3 478	$+\frac{1}{1158}$
3 624	3 628	$+\frac{1}{906}$
4 012	4 012	0
5 694	5 699	$+\frac{1}{1138}$
5 776	5 794	$+\frac{1}{321}$
5 987	5 992	$+\frac{1}{1197}$
6 061	6 068	$+\frac{1}{866}$
6 685	6 698	$+\frac{1}{515}$
58 610	58 688	$+\frac{1}{751}$

Each of the three tables gives the errors of each component section and its total ratio of error. In all, 293 640 m. (182½ miles) of stadia-measured boundary have been triangulated, in which the total difference in length is + 50 m., or an error of $\frac{1}{5873}$. However, this method of

stating the accuracy by aggregate lengths, though often used, is not the truest or fairest; such ratio of error should rather be a mean of all the different errors of closure of each short portion checked, giving that of each section its relative weight. The error is $\frac{1}{9000}$ when so considered.

The topography taken formed a belt 4 000 m. ($2\frac{1}{2}$ miles) wide for the whole length of the boundary and covered an area of about 1 700 sq. miles on the American side. The work as planned involved the instrumental location and determination of the elevation of all surface irregularities and the position of roads, trails, towns, ranches, wood, grass, and water. All such topographical points were taken as would appear on the maps drawn to the scale of $\frac{1}{20000}$, and with contours 20 m. apart.

The natural features of the country to be traversed and the conditions to be encountered plainly indicated the transit and stadia as the best field equipment. The necessity of reducing the size of the expedition to its lowest limit suggested the plotting of the maps away from the field. The topography, embracing a belt of country 4 000 m. ($2\frac{1}{2}$ miles) in width, required a general onward movement and a concentration of all instrument men and instruments on topography as mountain ranges were encountered, all working to advantage in limited areas. The occasional remoteness of the work from available camps, the very rough and difficult nature of many regions encountered, and the necessity of utilizing even the many very windy days called for the lightest and most compact equipment possible, while the precipitous character of many of the mountain ranges, requiring vertical angles of from 30° to 65°, frequently precluding the possibility of resection, and, at times, offering peaks for instrument stations that would barely furnish room for the tripod, confirmed the choice of the transit as the only practicable field instrument.

The party that measured the tangents was also charged with securing the topography adjacent to and within, perhaps, 200 m. on both sides of the boundary with greater detail than was used elsewhere in the topographical belt. A level line was run with exceptional care along the boundary to secure its profile; the elevations so secured also furnished elevations for the general topography. The tangent lines thus furnished in their directions, distances and elevations the basis of the topographical lines.

Where necessary and advantageous, transit lines would start at a point on a tangent line, extend in a general northward direction to near the northern edge of the 4000 m. zone, and turn again to the tangent line at another point, thus securing a check on their accuracy of azimuth, distance and elevation. The general course of procedure corresponded, in the main, with the usual practice in stadia surveys. Azimuths were carried continuously and measured the deviation from the meridian, or, more strictly, from a line at right angles to the tangent. As this involved two sights over each course to secure the azimuth, readings for distance and vertical angle were also taken on both foresight and backsight, giving a double determination on each course of its length and change in elevation. Topographical points were then located by side sights from these main-line stations, or, at times, from transit stations on unclosed spur lines. At times, when difficult mountain ranges were encountered, the parties engaged in measuring the tangents and carrying forward the level line were withdrawn from their usual work to reinforce the others, all parties then working advantageously together on pure topography. Where such concentration was not necessary, each party would carry on its own particular work. Nearly three-fourths of the total area was covered by this general method.

About 50 sq. miles was taken by a method of intersections. In the region so covered there were a few commanding peaks, below which lay the terrene in all its irregularity. The general procedure here consisted in the establishment of station signals on these commanding peaks, and from numerous stations on the boundary the tangent transitmen would take azimuths and vertical angles to these signals, from which the location and elevation of each station were computed; then these same stations were occupied, two at a time, in such order as to give the best intersections on points to be located. One engineer with a flagman, both mounted, where the country would permit, would traverse an advantageous area; the flagman signaled at each necessary point of the area, and the engineer made a contour sketch of the terrene, showing the position of the points from which the signals were made. At the same time the two transitmen at their stations would watch the flagman, and, on a signal from him, each would take the azimuth and vertical angle to his signal, held vertically on the ground. The engineer and the two transitmen would all note the time of each

sight, to have a check distinguishing corresponding observations. The similarity to the intersection method of locating soundings in hydrographic surveys is apparent; points are located in the same way, the difference being that on this topography the elevation of each point is also determined. Computation from the vertical angle of each transit applied to the scaled or computed distance gives two independent determinations of elevation.

While topography taken in this way is not capable of the accuracy of the usual method, it is still at times advantageous, and was used to facilitate and hasten the field work. Eighty per cent. of the points so located checked by the two computations within less that 3 m. in elevation, even when the sights were 10 000 or 12 000 m. in length; of those not so agreeing, that which was the true elevation was indicated by reference to the contoured sketch.

It was hoped in such intersection work that aneroid barometers might be used for obtaining the elevation. To test their accuracy they were used in the approved way, one being stationary in the field to note fluctuations in the atmosphere itself, while two, for comparison, were carried and read at each located point. Comparison of all three was made before and after work, and care used to secure good results. However, the test confirmed the author's opinion that they are useful only as reconnaissance or approximate instruments. The two carried and read simultaneously would sometimes differ 20 m., and elevations computed by comparison with the stationary barometer readings would at times differ from the instrumentally determined elevations by as much as 40 m. In this connection it may be of further interest to state that the mercurial barometer carried on the survey of 1849–1855 for determining elevations gave often very erroneous results, varying by different amounts from the true elevations to an extreme error of more than 400 m.

The stress of adverse natural conditions in the most inaccessible and forbidding portion of the boundary made necessary a further modification in the topography for an area of about 450 sq. miles. The region so covered combines obstacles to its penetration to such a degree that where traversed by the overland route for emigrants from Mexico to California, scores and probably hundreds died of thirst on the road. A portion of this region was considered absolutely impossible of penetration, and no expedition had traversed it. The sur-

vey of 1849–1855 attempted it twice and failed. In the face of this record, and with the summer heat of the desert approaching, it was considered necessary to expedite the work in every way possible, and the result proved the wisdom of the plan. With the increased celerity of progress due to the modified plan, the expedition was barely able to traverse this region and complete the work without delay and, perhaps, disaster.

The method used here was unchanged for the measurement and profile of the line and for the topography within 1 000 m. of the boundary. The modified plan affected only the belt from the 1 000 m. to the 4 000 m. distance, covering a zone 3 000 m. wide for the region in question. This belt was to be so taken as to show on the maps all natural features that could be delineated by hachures instead of by contours at the usual scale, $\frac{1}{30000}$, thus showing all the main characteristics, but necessitating a less refinement of work. The procedure consisted in locating and obtaining the elevation of flags placed on all prominent peaks and ridges from 1 000 to 4 000 m. from the line, by the transitman measuring the boundary. Another transitman would take azimuths and vertical angles from each of these located points on the other points in sight, thus covering the zone with a network of location lines. At the same time, he would take resections or stadia readings on the more important secondary features of the terrene, and complete the work by sketches. This method gave the instrumental location and elevation of the characteristic and principal natural features of the region, with approximation to the less prominent ones.

With these exceptions, the topography was secured, as before stated, by stadia location. In this work the average length of primary sights on the transit lines was 358 m., and that of the secondary sights, for general location and elevation, was 346 m. The average error of azimuth on the closed lines was 23″ per course. The total length of the 118 closed lines (not including the sections of tangents checked by triangulation and already given, and also excluding the section of tangent in each case furnishing the data for completing the closure) aggregated 826 516 m. (513½ miles). The average error of closure on these lines was $\frac{1}{752}$. The total change in elevation, or rise and fall, on these lines was 38 091 m. (124 970 ft.); the average error in closing of elevations was $\frac{1}{374}$ of the vertical component or change in elevation.

In considering these different results it should be borne in mind that the work was done with no effort to make a "record." On the contrary, the conditions were quite adverse, especially on account of the frequent excessive vertical angles, the vibration or boiling of the air, and the necessity of constant progress requiring work even in the numerous wind storms. It was the rule that stadia work should continue as long as it was a physical possibility for the stadia men to hold up the stadia rods against the wind. Elevations were carried on the closed transit lines from a level station at the beginning to another at the end. A discussion of the computation from one station to the next will illustrate the essential characteristics of all.

Consider the transit at a station of known elevation, taking an observation on the next in course. The interval is read, where convenient, on the stadia rod, the vertical angle is read for precisely this inclination, and the reading on the stadia rod of the middle cross-hair (for this same angle) is noted. The vertical component of the distance read, with its proper sign, plus for elevation and minus for depression angles, minus the recorded reading of the middle cross-hair, gives the quantity to be applied to the elevation of the instrument at the occupied station, to secure the elevation of the advance station, curvature and refraction being disregarded.

It is true that in stadia surveys it is often the practice to read the distance on a convenient part of the stadia rod, and then to change the telescope slightly before reading the vertical angle, until the middle cross-hair stands on the stadia rod at a height equal to the height of instrument; the purpose is to carry forward the elevations by simple distance-into-angle computation. This practice introduces error into even the horizontal (distance) component; while in the vertical (elevation) component, it introduces error varying with the space on the rod which the middle hair passes over during the change of the telescope. The other alternative, viz., to read the distance with the middle cross-hair pointing at the height of the instrument point of the stadia rod, would often be even more annoying, as it would restrict the length of sight to about 300 m. It is frequently necessary to take longer sights than this; it will be remembered that the average length of sight on the boundary survey was greater. Again, as the disturbing effect of refraction increases enormously toward the ground, as will be seen in the discussion of refraction following, it is not advisable to set the

lower hair within less than a meter (about 3¼ ft.) of the ground, even if the foot of the rod is unobstructed. It is true that if these practices are ruled out it necessitates the division of the stadia rod into true units, instead of fitting such divisions to the transit interval. This phase of the case is discussed later in the paper.

When the advance station is occupied, a similar process gives another determination of the station's elevation, and a combination of the two eliminates curvature and refraction as well as errors due to the instrument being in imperfect adjustment or due to the effects of heat upon it.* Thus, when no error is made in observation, and refraction remains constant from the time of foresight to the time of backsight, the equation

$$x = e + \tfrac{1}{2}(a + m + n - b)$$

will give the true elevation of the advance station, where

e is the elevation of the known station,
a is the height of the instrument at the known station,
m is the total vertical component of the foresight,
x is the elevation of the unknown station,
b is the height of the instrument at the unknown station, and
n is the total vertical component of the backsight.

The average error in closing before the adoption of this formula was more than a half greater than after its use.

For the purpose of determining the error of closure in elevation relative to the degree of vertical angle, the author grouped the lines according to the size of those angles and plotted the corresponding errors as ordinates. The equation of the curve so formed, where α represents the average vertical angle in degrees is, for meters per kilometer,

$$\text{Error} = \frac{30\,(\alpha\,.\,\text{tan.}\ \alpha) + 8}{100}$$

or, expressed in feet per mile,

$$\text{Error} = \frac{30\,(\alpha\,.\,\text{tan.}\ \alpha) + 8}{19}$$

It would seem that the error should vary about as the tangent of the angle only. The presence of α itself in the equation must be due to the fact that in practice, stadiamen will not hold the stadia rod

* Discussed by the author in the *Journal* of the Association of Engineering Societies, August, 1893.

exactly vertical, even with the aid of a plumb line, the tendency being to hold it perpendicular to the line of sight, and an error varying with its inclination is thus introduced.

More favorable conditions in the field would change the constants of the equation and give a smaller probable error. Considered with reference to results with wye levels, this error is large, though not so surprising when the rise and fall of the lines is remembered. It was greater than it should have been because the reduction formula before given was not devised until a part of the survey had been completed, and because work was often continued when wind storms rendered it impossible for the stadia rods to be held steadily for the reading. A greatly increased accuracy could be readily secured if the vertical circles of transits were made to read to fractions of a minute, if the telescope bubble-tube were made and ground with as much care and refinement as that of a wye level, if a bubble-tube were attached directly to the vertical circle vernier, as has been done on the Lake Survey and Mississippi River Survey, instead of trusting to the plate bubble, and if stadia hairs are made as fine as possible consistent with distinctness.

Here the author would again call to mind the fact that on the boundary survey the usual stadia methods were generally followed. Some modifications were found necessary because of unusually severe conditions of work and a description of them follows; these refinements were employed to secure more accurate results where such conditions would have unduly magnified the smaller errors that generally compensate in time, and so are usually disregarded. The desirability of applying the following refinements must be decided by the circumstances of each case.

In determining the interval factor of a transit's stadia hairs, whether for reduction or for marking the stadia rods correspondingly, especial care should be used to secure as close an approximation to average working conditions as practicable. This includes a consideration of the time of day and the elevation of the line of sight above the ground to involve the average amount of refraction; the length of sight, varying from the shortest to the longest usual length; frequent tests of the interval; readings by the habitual user of the transit to involve his personal equation; and great care in the measurement of the base for the determination, numerous readings at the different distances, frequent determinations and carefulness in all the details.

The effect of differential refraction on the determination is well stated in a paper by Mr. L. S. Smith, of the University of Wisconsin, and published by that University in its engineering *Bulletin*, Vol. I, No. 5. Mr. Smith shows from extended experiments that the amount of refraction of the line of sight of the upper stadia hair usually differs from that of the lower one. Sometimes this differential refraction increases the intercept and at other times diminishes it. Early in the morning, during the summer in Wisconsin, and up to about 8.30 A. M., the effect of refraction on a stadia reading is to increase the intercept, and so the distance beyond the average reading. From that time to about 2.30 P. M. the intercept is less than the mean, and from this time until night the intercept again becomes too great, as in the early morning. Fig. 1 shows a typical case. It illustrates how

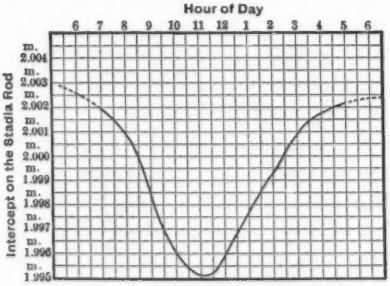

FIG. 1.

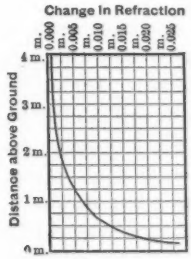

FIG. 2.

differential refraction, though slight in amount, does and must affect the intercept. The curve must be taken merely as suggestive, for its ordinates vary with the state of the atmosphere and ground, temperatures, weather, latitude and change of the seasons. A variation of these conditions appears to affect the hours and amounts of the maximum and mean distortions, and perhaps the direction of curvature itself, while not changing the fact that at certain times of day refraction is abnormally great and at others abnormally small. This principle last expressed is the one affecting stadia intercepts, and should have great weight in fixing the proper time and method of interval determination. Mr. Smith also found that within 1 m. of the ground the effect of refraction is greatly increased, resulting in increasingly distorted intercepts as the ground is approached. Fig. 2

illustrates a typical case clearly, showing the increased error nearer the ground, which seems to occur always.

The stadia interval should, then, be determined when the lower cross-hair does not come within 3 ft. of the ground, and should involve conditions which will secure the average intercept. These conditions are so various and the amount of differential refraction at any time so uncertain that it becomes a necessity to determine an interval through all the hours of day which will constitute the working hours. Nor should a single determination be accepted as final, for, with all care in choice of conditions, unusual circumstances or changed conditions might be present to affect the result. No one should plan an extensive triangulation from a base measured only once. Determinations should be made at reasonable intervals of time with equal care, the mean of which is much more likely to secure average conditions, and so a truer value.

Neglect in regard to securing average conditions of refraction have proved, unwittingly, very troublesome to those who have not considered it. The author calls to mind an extensive and costly topographical survey where the interval was determined from a single series of about 10 sights taken at about 11 A. M., and used for several years as so obtained. From Mr. Smith's investigations it is seen that this hour is one of the most unfavorable for securing average conditions of refraction, and will introduce error in the unit of measure itself. In this case it would make the survey distances too great. This is in fact what did occur, the engineer in charge noting the resulting errors without appreciating their true significance. The use of a transit for months and years without again testing its interval is a mistake, both because an increased number of determinations gives greater reliability to the mean, and because changes in the cross-hairs, though rare, do sometimes occur. The author has the record of an interval which changed gradually, though not uniformly, more than 2% in a year. Engineers should be sure of their instruments. In leaving this subject, the importance of intelligent and careful detail should be emphasized, as it is the basis of all stadia work.

The consideration of focal distance plus distance from the objective to the center of instrument, $f + c$, is often discussed. By an optical law applied to the construction of the transit telescope this quantity, greater than 1 ft., must enter to give correct results. There is no reason why the law of optics and sound theory should be disregarded,

and it is the usual custom either to add $f + c$ to the sight or to so determine the interval factor that by omitting this $f + c$ distance from the base measurement, the $f + c$ is eliminated for a mean distance. This latter procedure, however, gives erroneous readings for distances greater or less than the mean length of sight fixed upon, and therefore should not be used for the more accurate stadia work.

The author believes that the best practice (in the interval determination) is for the transit to be set at a distance equal to $f + c$ back from the beginning of the base and then to add $f + c$ to each distance read. By using $f + c$ in this straightforward way, an increased accuracy is given to all the work, and especially to the measurement of comparatively short distances, where the ratio of error is at best always greatest and where there is need of care. An exception to either of these usages occurs in the article in the *Journal* of Association of Engineering Societies, January, 1893, on the survey of St. Louis, the writer of which advocated the determination of the interval in such a way that $f + c$ entered into the base measurement and then in practice ignored that quantity. The stated reasons for neglecting $f + c$ was that in the computation of the lines that writer found the distances were almost invariably read too great. However, this is not a necessary error of stadia work. Other surveys do not find these constant cumulative plus errors in distance; the tables of distances given previously in this article happen to have a greater aggregate length of line where the stadia distance was read too short than the total in which it was read too long. It seems that this illustrates again that it is bad practice to ignore sound principle. If an explanation of these constant plus errors of the St. Louis survey is sought, it will be found to occur, not as a retribution for not juggling with $f + c$, but in the probability that the determination of the stadia interval itself was made in such a way or at such a time that refraction crept in as the disturbing element.

To have the stadia rod divided into true unit lengths instead of into approximate units to fit its particular transit is desirable for many reasons. It makes impossible any error due to an accidental interchange of stadia rods among transits; any stadia rod not especially constructed for the particular transit may be used in case of emergency or desirability, viz., level rods may be used for stadia readings whenever desirable, as advocated by J. B. Johnson, M. Am. Soc. C. E., for stadia railroad preliminary surveys, and by Mr. Benjamin S. Lyman; the

stadia rods may be used for level rods or for any direct measurement when desired; or stadia rods and stadiamen may be transferred from one party to another where conditions require, and no error results. Two considerations already mentioned make this division into true units almost a necessity; the one that the distance above the stake at which the middle hair stands when the vertical angle is measured may be read in true linear distance; and the second, that error in the stadia interval, occurring either from a change in the cross-hairs or from a subsequent determination indicating that previous determinations have not developed the exact and true factor, may be corrected without the complete change of the stadia rod divisions, which would, at best, consume several days of delay and careful work, and might be impracticable.

Stadia.	Distance.	Stadia.	Distance.	Stadia.	Distance.
.001	.10	0.6	64.10	2.8	294.66
.002	.21	0.7	74.58	2.9	305.14
.003	.31	0.8	85.06	3.0	315.62
.004	.42	0.9	95.54	3.1	326.10
.005	.52	1.0	106.02	3.2	336.58
.006	.63	1.1	116.50	3.3	347.06
.007	.73	1.2	126.98	3.4	357.54
.008	.84	1.3	137.46	3.5	368.02
.009	.94	1.4	147.94	3.6	378.50
.01	1.05	1.5	158.42	3.7	388.98
.02	2.10	1.6	168 90	3.8	399.46
.03	3.14	1.7	179.38	3.9	409.94
.04	4.19	1.8	189 86	4.0	420.42
.05	5.24	1.9	200.34	4.1	430.90
.06	6.29	2.0	210.82	4.2	441.38
.07	7.34	2.1	221.30	4.3	451.86
.08	8.38	2.2	231.78	4.4	462.34
.09	9.43	2.3	242.26	4.5	472.82
0.1	11.70	2.4	252.74	4.6	483.30
0.2	22.18	2.5	263.22	4.7	493.78
0.3	32.66	2.6	273.70	4.8	504.26
0.4	43.14	2.7	284.18	4.9	514.74
0.5	53.62				

The division of the stadia rod to fit the transit is widely used and advocated. The reason for this is found in the fact that the observation gives the distance if $f + c$ is disregarded in the interval determination; or it gives the true distance by the simple addition of $f + c$ to the reading where the interval is correspondingly determined; while a true unit division is supposed to involve for each sight a complex reduction, taking both time and care. For example, if a transit has the stadia hairs so spaced that at a distance of 100 ft. the intercept is 0.9542 ft., or, in other words, its interval factor is 0.009542, it is generally supposed necessary to divide each reading taken on a true unit stadia rod

by 0.009542, and then add $f + c$, entailing a great amount of work. However, that supposition is not true. On the boundary survey, Mr. P. D. Cunningham devised a method whereby the true distance is derived with no more computation than is necessary when the untrue unit division of the stadia rod is made. The method is, in brief, as follows: A table is made (illustrated on the opposite page for the shorter distances with an interval factor of 0.009542 and a value of $f + c = 1.22$) giving readings for all distances from 0.1 up to the longest, in increments of 0.1. These are computed by dividing each distance wanted by the interval factor, and adding $f + c$, viz., for a rod reading of 3.4 the corresponding tabular distance is $\frac{3.4}{0.009542} + 1.22$. Similar computation is also made for distances from 0.01 to 0.09, differing from each other by 0.01, and for distances from 0.001 to 0.009, differing from each other by 0.001, in a way exactly similar to the above, except that $f + c$ is not added. Then the true distance corresponding to any stadia rod reading is found by a simple addition of quantities taken from the table. Thus, if the reading is 2.67, the true distance is 273.70, opposite 2.6, plus 7.34, opposite 0.07, which equals 281.04.

FIG. 3.

If a slide rule is accessible, each stadia rod reading can be divided by the interval factor and the corresponding true distance read from it, to which $f + c$ must be added; but this would involve more work than the use of a table. The table itself can be formed in half an hour by the aid of the slide rule, or in two or three hours without. By using such a table there occurs no more computation than is necessary when the rods are not divided into true units, and it permits all the advantages previously given for the correctly divided rod without occasioning any disadvantage.

The design of stadia rod used on the boundary survey is the metric one given in Fig. 3. The stadia rod with the foot divisions (Fig. 4) has been extensively used by the author on internal improvements and surveys. In

both Figs. 3 and 4, the hatched divisions represent red divisions on the rod. It is the general opinion that stadia rods should combine simplicity, clearness, distinctive divisions and subdivisions, and efficiency in indicating the shorter distances very accurately and in giving great distances readily. Those illustrated were designed with these characteristics in view, and their use confirmed their expected utility. With an erecting telescope at a distance of 250 m., the centimeter divisions could be seen and spaced by the eye into millimeter readings, while the decimeter divisions could be seen at a distance of 700 or 800 m., and correspondingly spaced into centimeters.

FIG. 4.

On one portion of the boundary more than 100 miles intervened between two successive points as marked in 1855. The line of sight between these points was obstructed by two mountain ranges, one 45 miles and the other 60 miles distant, and it became necessary to establish a station on the connecting line on the nearer range. This preliminary work was accomplished by successive approximations. Heliotropes were shown at the boundary points and a transit and a heliotrope were placed on each of the ranges. On one range a trial point was assumed and the transit on the other range lined in between it and the adjacent boundary heliotrope. When this was accomplished the transit was replaced by a heliotrope and the transitman on the first range lined in between that heliotrope and the boundary heliotrope adjacent to him. Repeating this operation time after time, a station was finally established on the nearer range. When on the regular survey this line so determined was prolonged with a theodolite and with all care, it was found that the established point was 0.87 m. from the true line, or had an angular error of 2".45. So close a result was exceptional, but it is given as indicating the reliability of ordinary transits and heliotropes when used with care and skill.

Such, then, are the methods, results and conclusions of this survey. They occurred under adverse circum-

stances necessitating boldness in length of sight, vertical angle, and in general details. Interpreted in this light, they indicate the results of probably extreme conditions, certainly not average ones. Its use and usefulness make the stadia system so extensively employed in continental Europe on topography and on surveys for railroads, canals and other public works deserve all the growing attention it is receiving, and more. In general economy, adaptability and efficiency it is unequaled.

DISCUSSION.

Mr. Abbot. F. V. ABBOT, M. Am. Soc. C. E.—The rivers in South Carolina are commonly bounded on both sides by extensive cypress swamps, and it is generally impossible to set up an instrument anywhere on the banks, which are overhung with brush. The mud is of indefinite depth. The speaker had a number of surveys made of these streams, using the stadia method and employing two boats. The transit was lashed firmly in the bow of one boat, so that it would not fall overboard. The men in the second boat tied a piece of cloth on a tree overhanging the water on the opposite side of the river and as far down stream as they could be seen from the transit boat, and held up a stadia rod directly against the rag. The rod was then read and the magnetic reading also taken from the transit boat, which then went down stream past the rod boat as far as it could and still secure a sight of the tree. From this new position a stadia reading and the magnetic bearing were taken of the rod held in the second boat at the rag. The rod boat then went down the river as far past the position occupied by the transit boat as it could be seen, and the distance and magnetic direction were read again. The transit boat was always on the left bank, and the rod boat on the right bank of the river. By repeating this process, it was possible to secure a very fair map of such a river, obtaining its width and length with more accuracy than in any other way with which the speaker was acquainted. Three men in each boat can survey 30 miles of river a day (down stream) without any great trouble if no soundings are made.

Mr. Webb. W. L. WEBB, Assoc. M. Am. Soc. C. E.—In using the stadia to measure distances between stations, the speaker had adopted a method of reducing the error in reading the rod with the upper and lower cross wires. After the reading with the three wires is taken, the tangent screw of the vertical motion is turned very slightly, just enough to give different readings. Theoretically a change in the vertical angle will produce a slight difference in the length of rod subtending the wire interval, but practically the difference is very small and may be

Mr. Webb. neglected. For example, with a vertical angle of 10° and a distance of 1 000 ft., the vertical angle would have to be changed 10′ in arc to make a difference of 1 ft. in the apparent horizontal distance and 10′ of arc in 1 000 ft. would mean a movement of 2.9 ft. of the wire over the rod. Entirely different readings are thus obtained and there is no tendency to repeat involuntarily an error made in the first set of observations. Any number of sets of readings may be taken in this manner, and the average will be much more reliable, especially for long sights, than a duplicate reading of the rod without any change in the vertical adjustment of the telescope.

CORRESPONDENCE.

Mr. Allen. KENNETH ALLEN, M. Am. Soc. C. E.—The prevailing opinion regarding the use of the stadia has been heretofore that, at best, it would answer as a rough expedient, comparable rather to pacing than to chaining as regards accuracy. Among those familiar with its use, however, its great value has been recognized and its adoption advocated where single errors of $\frac{1}{300}$ to $\frac{1}{1000}$ would be allowable. The details of the work may vary greatly, but from a considerable experience the writer wishes to emphasize his approval of two of the author's recommendations.

First.—The rod should be divided to true units, feet or meters, and decimals. Beside the reasons given in the paper, the writer has found it preferable to take the levels directly when practicable instead of by the vertical arc, which can only be done by means of such rods. If no means are at hand to provide a special rod a Philadelphia level rod will answer nearly as well for distances under 1 000 ft. in length.

Second.—Stadia hairs should be adjusted with $f + c$ added to the distance, and this amount added to all observed distances. This does away with the use of calculations or tables, and, so far as it goes, is correct.

But some rather recent surveys, including that described by the author, indicate that the stadia may be used for work of a much higher grade by making due allowance for refraction. The writer would also recommend for this purpose telescopes of higher magnifying power than those in ordinary use. The very great rapidity and economy attained by the use of the stadia make every increase in its adaptability a matter of importance, and the writer believes such an increase to follow from the carefully conducted investigations of the survey described.

The aneroid is believed to be deserving rather more credit than would follow from its use on this survey. The climatic conditions may have been peculiarly unfavorable. Large individual errors may occur under the most favorable circumstances, but with the precau-

tion of using several instruments and making the proper reductions, the writer is surprised at the discrepancies found. It has been his practice to use several aneroids as checks on one another, but the following errors, determined some four years ago in western Pennsylvania from observations at points of known elevation, were (with the exceptions noted) from the mean of two observations taken with a single aneroid. The extreme range of elevations being comprised between 865 and 1 339 ft. above sea level, the results are of course not comparable to mountain work, but serve as a sample of the accuracy of such determinations in ordinary hilly country. Mr. Allen.

Station...	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
	0*	−13	+6	−17*	+2*	−1	−2	+2	+5	−2*	+7	−8*	−19*	+12	−2

Station...	16	17	18	19	20	21	22	23	24	25	26	27	28	29
	+36*	+10	+5	+32	+19	0*	0	+11	+5	−16	−20	−19*	−12	0*

WILLIAM B. LANDRETH, M. Am. Soc. C. E.—The paper calls to the writer's mind some work done on a stadia survey in the states of Sinaloa and Durango, Mexico, in 1881 and 1882, by H. M. Wilson, M. Am. Soc. C. E., and the writer. Mr. Landreth.

Edward P. North, M. Am. Soc. C. E., then chief engineer of the Ferro Carril Sinaloa y Durango, in looking up the matter of a possible railroad route from the Pacific coast, at Altata, Mex., to the city of Durango, found that there were no reliable surveys or maps of the country between those places, and to obtain some information thereon, decided to have a survey made of the principal mountain trails between them. As the examination was to be of the nature of a reconnaissance, it was deemed advisable to use the stadia to reduce the cost and time of making the survey. Two routes were to be taken from Culiacan; a southern one, passing through the towns and villages of Las Tapias, Santa Cruz, Las Vegas, Cosala and Vivorillias to Durango; and a northern one, returning from that city through Canitlan, Guatimape, Santiago, Santa Catalina and Canelas to Culiacan. The two routes chosen were those followed by pack trains crossing the mountains, and would give a closed survey, affording a means of approximately checking the accuracy of the work.

Each party consisted of an engineer, who acted as transitman, and native rodmen, stake drivers, machete men for cutting brush, and a mule driver. Mules were used for carrying the provisions and clothes of the party, and no tents or outfits for permanent camps were provided, owing to the rapidity necessary in the work. A party in charge of the writer started on the southern route early in October, 1881, from the city of Culiacan, with instructions to follow the trails generally taken by pack trains between the villages, and to report progress whenever possible, by means of travelers or mule trains met on the road.

The transits used were made by Messrs. Heller and Brightly, with

* From single observations.

Mr. Landreth.

stadia hairs set to cover 1 ft. between upper and lower hairs on a Troy leveling rod when set 100 ft. plus a constant distance from the instrument. The constant added was frequently checked, but it was found that the stadia hairs needed little adjusting.

The rodman, accompanied by an experienced Indian stakeman, set wooden hubs in the angles of the trail, and after the readings were taken, left a small peeled sapling with a piece of white paper attached near the hub, so that the transitman could easily find it on coming up; he then went ahead to locate the next one. The transitman set up over the hub, turned back on the sapling at the last hub for a backsight, took the readings, placed the sapling behind the hub, and went on to the next one. This method dispensed with the services of a rear flagman, and it was determined by experiment, before adopting it, that none of the accuracy of the work was sacrificed by so doing. The readings included the angle of total deflection from a base line in Culiacan, magnetic bearing, reading of upper, middle and lower cross hairs, foresight with level when possible, vertical angle, a measurement of the height of the instrument from hub with tape, and aneroid and thermometer readings.

From Culiacan to Cosala the route lay among small foothills and crossed the valleys of one large and several small streams, and the thick underbrush made the progress slow. Soon after leaving Cosala, the narrow, rocky gorge of the San Lorenzo River was entered, and followed for 40 miles to the Sierra Madre Mountains at Vivorillias, where the trail rose 6 000 ft. in about 10 miles, to an elevation above the sea of 11 000 ft. From the summit a descent was made into long mountain valleys, which were followed for about 120 miles to the city of Durango. Durango is a place of about 30 000 inhabitants, lying in a wide plain at an elevation of 8 000 ft. above the sea. After a week's rest at Durango, writing a report of the trip, repairing the camp outfit, and giving the pack animals a chance to recover flesh and vitality, a start was made on the northern route December 26th. For the first 120 miles, to Santa Catarina, the route was through wide plains and open valleys, passing several populous towns and villages. This portion was well adapted to fast work, and 15 miles per day was averaged for several days.

In accordance with orders by wire from Mr. North, received while in Durango, the writer waited at Santa Catarina for Mr. Wilson's party, which had left Culiacan later and had taken the northern route. Mr. Wilson soon arrived, the lines were joined, and the return on horseback to Culiacan commenced. Mr. Wilson intended to examine the head waters of the Humaya River, but the total absence of trails and the fact that all of the pack and saddle animals were so feeble from the effects of cold and snow that they could scarcely carry a load on a good, well-beaten trail, caused him to abandon that project and

return by the route of his survey with the writer. Heavy rain and snow storms were encountered, and progress was slow, as it was necessary to wait for several streams to subside before they could be forded. Mr. Landreth.

The writer is of the impression that the circuit covered was 606 miles, that the latitudes and departures when calculated closed within 1 100 ft., and that the total cost of the survey was about $3 per mile.

As the survey started nearly at the sea level, crossed over mountain passes at an elevation of upwards of 11 000 ft., was through rough, broken country as well as level plains, and through tropic heat and winter's snow and ice, it furnishes an example of the adaptability of the stadia to such work.

J. B. JOHNSON, M. Am. Soc. C. E.—This paper is another endorsement of the stadia method of surveying, which also adds materially to the advance of the system. The fact that the stadia method was found by actual trial in easy country to be more accurate than chaining, even after the correction of several large errors caused by a miscount of the number of chains, is very significant. This has often been claimed by the advocates of this method, but probably never before proved so directly by actual trial. Mr. Johnson.

Another significant contribution to the literature and also to the practice of this method is the evidence produced of the importance of the time and method of determining the value of the wire interval, or of graduating the stadia rod. This matter has never before been clearly understood. Mr. L. S. Smith has rendered the users of the method a good service in his investigations in differential refraction, in showing the importance of choosing average conditions for standardizing the instruments.

The author's methods of work are to be commended for their accuracy, but they require greater labor of reduction than the ordinary methods. Thus it is common to read the distances directly from the rod, specially graduated for that purpose, and to read the middle wire to the "H. I." mark on the rod for vertical angle. These methods have been found satisfactory in general, and will probably continue to be used. The most important errors in the ordinary practice are a failure to graduate the rod for many readings taken under all sorts of atmospheric conditions, and reading the bottom wire too near the ground.

By far the most rapid and accurate method of reducing stadia notes for differences of elevation is by means of the Colby slide rule, which is specially constructed for this purpose. By using this the differences of elevation can be taken out as rapidly as the notes can be called off and the results written down by the assistant. This rule is equally adapted to all units or combination of units, giving elevations in feet for distances in yards or meters, or *vice versa*, as readily as though all were in the same unit of measure.

Mr. Van Ornum

J. L. VAN ORNUM, Assoc. M. Am. Soc. C. E.—Captain Abbot's description illustrates the adaptability of the stadia in another direction, to overcome difficulties. It is most useful in hydrographic surveys. The author has made use of the stadia on surveys of different rivers for locating the banks, and also for locating soundings varying in extent from channel depths to a complete gridiron of lines covering the whole river.*

Concerning Professor Webb's suggestion of increasing the accuracy of readings, the author would state that his own method has always been to read the intercept between the stadia hairs instead of the reading of the three cross-hairs themselves. This method reduces the readings in the field from three to one and saves the subsequent reduction to intercept. The author's experience has been that there is little need of the verification secured by reading each of the three cross-hairs, and time and effort are saved. Furthermore, in some cases reading the three cross-hairs instead of observing directly the intercept would bar the use of the stadia entirely on account of the greater deliberation necessary ; in locating soundings, for example, it is often necessary to make the distance reading in one or two seconds.

Undoubtedly the discrepancies in the barometer work given in the paper are surprisingly large, and yet with such erroneous results occurring in the hundredth case, despite the care used, it would seem that complete and entire reliance cannot be placed on barometer elevations until verified by other and accurate means.

Many engineers question the advisability of using adjustable stadia hairs, as mentioned by Messrs. Allen and Landreth. There is certainly a chance of their changing their position, and if this should occur there would be introduced a corresponding error in all the work.

The author is inclined to think that Professor Johnson overestimates the extra labor involved in using the methods described in the paper. While wishing to be understood as not attacking the usual methods of specially graduating the stadia rods for each particular transit and of reading the vertical angle to the "H I" mark on the rod (methods which have proved very serviceable and generally accurate enough), he believes the methods given in the paper offer advantages for any survey, and in difficult work become a real necessity in eliminating errors that should hardly be allowed, viz., at a distance of 1 000 ft., with the usual stadia rod the intercept would cover so much of the rod that in changing the telescope to read the vertical angle to the "H I" point probably 3 ft. would be passed over in the movement. This, for a vertical angle of 20° would introduce an error of 2.25 ft. into the horizontal and of 1.95 ft. into the vertical component. Severe conditions may not often occur, but when encountered they may be overcome without a sacrifice of accuracy.

* See the *Journal* of the Association of Engineering Societies, Vol. XIV, No. 3.

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THE PHYSICAL QUALITIES OF ACID OPEN-HEARTH NICKEL STEEL, AS COMPARED WITH CARBON STEEL OF SIMILAR TENSILE STRENGTH.

By H. H. CAMPBELL, M. Am. Soc. C. E.

READ AT THE ANNUAL CONVENTION, JUNE, 1895.

The strength of a bridge of a given weight and design, the weight of a bridge of a given strength and design, and the longest span possible under a given design, are all limited by the elastic strength of the material used. These well-understood axioms point naturally to the use of steel possessing a high elastic limit, but this tendency is opposed by the no less well-known fact that, as the elastic strength of steel is increased by ordinary strengthening influences, there is a reduction either in the static or the shock ductility. It is true that an increase in brittleness under shock is not always shown by the ordi-

nary system of tests, but broad generalizations from general experience have led engineers to avoid metal of high strength. Within recent years certain experiments by the United States Government upon armor plate have proved quite conclusively that an alloy of nickel with steel will give a metal possessing a high ultimate and elastic strength with an exceptional toughness. Moreover these experiments seem to show that the metal is pre-eminently fitted to withstand the effects of sudden shock.

The manufacture of nickel steel is attended by a considerably increased cost, arising from the cost of the nickel itself, which, although quoted at the lowest price that has ever been known, is now sold at 35 cents per pound. Inasmuch as the steel should contain about 3% of this element, it is easy to see that the cost, under the present conditions of nickel production, must prohibit the use of the new alloy for ordinary purposes. It has probably occurred to many engineers, however, that under certain circumstances, as in the construction of long-span bridges, the extra cost per pound might be much more than counterbalanced by the diminution in the weight, particularly of the tension members.

Before venturing to use such a new material, the conservative engineer would ask for some definite information on its physical qualities when rolled into plates, angles and rods, under the conditions of everyday practice. To furnish this information the Pennsylvania Steel Company has made a heat of nickel steel, and has rolled it into common structural shapes, the results being herewith presented. It has also made similar shapes of low phosphorus acid open-hearth steel without nickel. These two kinds of steel were rolled in the same mills, and were tested in the same machines and by the same operator, so that the results are fairly comparable. In order that such an investigation should be conclusive, it would be necessary to make a large number of heats and several tests from each heat, but that is almost out of the question in the case of the nickel steel on account of the expense involved and the fact that there is no present market for the product. On this account only 4 tons of metal were made.

Two ingots were cast of the nickel steel, one of which had a cross-section of 18 x 20 ins., and the other 16 x 20 ins. The larger ingot was rolled into four slabs with a cross-section of 16 x 5 ins., and two of these were rolled in the universal mill of the Central Iron Works,

Harrisburg, Pa., and two were rolled in the plate mill of the Paxton Rolling Mills, Harrisburg, Pa. Both mills made one ⅜-in. plate and one ¼-in. plate. The amount of reduction on these slabs from the ingot was not as great as might be desired, and it would have been better if a larger ingot and a thicker slab had been used; but, as the whole charge weighed only 4 tons, a larger ingot was out of the question. The smaller ingot was rolled partly into blooms and partly into billets. The blooms were rolled by the Pennsylvania Steel Company into angles of different sizes, while the billets were rolled into rivet rods at the same works.

The rods, angles and plates of the high steel containing no nickel, which is called "hard forging steel" in the tables, were rolled from such blooms and slabs as happened to be on hand at the time, and hence were from different charges. They were made from ingots varying in cross-section from 16 x 20 ins. to 24 x 32 ins.

There are also given the results obtained on a lot of steel which was used for the construction of the massive gates now being placed in the Cascade Canal locks on the Columbia River, in Oregon. There are four of these gates, each consisting of two leaves, each leaf being 51 ft. wide and from 37 to 55 ft. deep. The steel was made by the Pennsylvania Steel Company, while the gates were built by the Maryland Steel Company. The fact that the metal was of unusual strength will render the figures valuable for comparison. The specifications required that one piece of an angle should open out flat, while another piece should close shut without fracture. In the case of plates, a strip was to bend double around a curve whose radius was equal to 1½ times the thickness of the piece. As a matter of fact, each test was bent flat upon itself, and out of 3 000 tons of material there was not a failure of a test piece of angle or of plate. The specifications called for an ultimate strength between 70 000 and 80 000 lbs. per square inch, but the accompanying table gives only those heats that ran between 75 000 and 80 000 lbs. This metal is called "forging steel" in the tables.

Tables Nos. 1, 2 and 3 give the detailed records of the three grades of steel, while Table No. 4 places these records in more convenient form for comparison. Table No. 5 gives the result of applying some of the ordinary miscellaneous tests of structural steel. The elastic limit was found by the drop of the beam. This was readily determined in the case of the nickel steel and of the softer "forging steel," but was

not well marked in the plates of the "hard forging steel." In the case of the "forging steel" there are some data lacking. This arises from the fact that the figures given are records of steel made and tested more than a year ago, and there was no need at the time of having this particular information. The results are rather disappointing in showing a less superiority of the nickel steel than may be commonly supposed, Special methods of heat treatment might be made to show better results, but if this plan is followed in the case of one grade of steel, it should be followed in all others. Judging from the records given, it seems doubtful if nickel steel offers such marked improvement in quality that it can be employed profitably in ordinary engineering work.

TABLE No. 1.

Physical Properties of Acid Open-Hearth Nickel Steel.

Made by the Pennsylvania Steel Company. Composition, C, 0.24; Mn, 0.78; S, 0.027; P, 0.032; Ni, 3.25.

Shape of member.	Size of member.	Ultimate strength. Pounds per square inch.	Elastic limit. Pounds per square inch.	Elastic ratio. Percentage.	Elongation in 8 ins. Percentage.	Elongation in 2 ins. Percentage.	Reduction of area. Percentage.
Rounds	¾" diam.	86 220	64 140	.....	20.00	34.00	45.6
	"	85 960	63 100	.....	20.00	34.00	45.5
	"	85 720	63 580	.....	20.50	34.00	48.1
	"	86 160	63 480	.....	20.25	34.00	46.1
	Average..	86 015	63 575	73.9	20.19	34.00	46.3
Angles	3" x 3" x ⅜"	88 600	59 720	.....	19.75	37.00	49.2
	"	85 280	57 800	.....	22.00	41.00	51.5
	3" x 3" x ½"	87 000	58 140	.....	23.50	41.00	50.7
	Average..	86 960	58 553	67.3	21.75	39.67	50.5
Universal plates, Longitudinal tests	⅜" x 16"	84 080	57 320	.....	21.25	40.00	53.1
	"	85 140	59 240	.....	21.75	39.00	54.2
	"	86 480	59 000	.....	19.25	37.00	47.8
	½" x 16"	86 200	58 260	.....	21.00	40.00	54.1
	"	86 660	58 320	.....	21.00	39.50	52.3
	"	86 080	58 320	.....	22.25	40.00	50.5
	Average..	85 773	58 410	68.1	21.08	39.25	52.0
Universal plates, Transverse tests	⅜" x 16"	84 460	57 560	.....	14.50	27.00	40.6
	"	87 020	58 440	.....	14.50	26.00	36.8
	"	87 440	59 060	.....	15.75	31.00	39.1
	½" x 16"	86 300	58 160	.....	18.50	34.50	41.6
	"	86 360	57 900	.....	16.25	25.00	25.6
	"	86 920	58 160	.....	19.50	30.00	33.0
	Average..	86 417	58 203	67.4	16.50	28.92	36.1
Sheared plates, Longitudinal tests	⅜" x 44"	86 520	59 560	.....	20.50	37.00	52.6
	"	84 440	58 800	.....	19.50	37.00	48.8
	"	86 680	59 640	.....	17.00	31.50	42.8
	"	85 640	57 600	.....	16.75	32.50	40.0
	½" x 44"	84 020	57 600	.....	20.50	39.00	52.2
	"	84 880	57 180	.....	19.75	37.00	51.5
	"	85 180	56 800	.....	19.00	34.50	50.2
	Average..	85 337	58 169	68.1	19.00	35.50	48.3
Sheared plates, Transverse tests	⅜" x 44"	85 120	57 680	.....	17.00	26.00	34.3
	"	83 840	58 380	.....	19.00	34.00	42.5
	"	83 630	58 040	.....	18.50	35.00	42.8
	½" x 44"	83 520	55 220	.....	16.00	34.00	48.0
	"	84 520	56 980	.....	15.50	32.00	47.0
	"	85 630	.....	.....	16.75	34.00	45.7
	Average..	84 377	57 260	67.9	17.13	32.50	43.4

TABLE No. 2.

Physical Properties of Acid Open-Hearth Hard Forging Steel.

Made by the Pennsylvania Steel Company. Composition, C, 0.30 to 0.35; Mn, 0.60 to 1.00; S, 0.03 to 0.05; P, 0.03 to 0.05.

Shape of member.	Size of member.	Heat number.	Ultimate strength, Pounds per square inch.	Elastic limit, Pounds per square inch.	Elastic ratio. Percentage.	Elongation in 8 ins. Percentage.	Elongation in 2 ins. Percentage.	Reduction of area. Percentage.
Rounds	¾" diam.	11 114	87 900	58 630	.....	15.90	23.50	30.5
	"	11 114	87 180	57 140	.....	15.75	24.00	30.3
	"	11 114	87 500	57 940	.....	18.50	27.00	32.0
	"	11 114	88 070	58 510	.....	16.63	23.25	28.3
		Average..	87 663	58 055	66.2	16.70	24.44	30.3
Angles	4" x 4" x ¾"	11 114	88 000	52 640	.....	19.00	35.50	47.2
	4" x 4" x ½"	11 114	85 740	56 980	.....	20.50	38.00	50.9
	5" x 3" x ½"	11 114	89 720	52 840	.....	18.25	31.00	31.7
		Average..	87 820	54 153	61.7	19.25	34.83	43 3
Universal plates; longitudinal tests	½" x 16"	11 233	82 900	50 440	.....	20.75	37.50	46.1
	"	11 233	83 640	50 500	.....	19.50	36.00	43.9
	"	11 233	81 780	49 550	.....	21.25	39.50	50.9
		Average..	82 773	50 163	60.6	20.50	37.67	47.0
Universal plates; transverse tests	½" x 16"	11 233	84 940	(50 000)*	.....	19.00	23.00	27.3
	.....	11 233	84 500	(50 000)*	.....	19.00	24.00	27.7
	.....	11 233	86 080	(50 000)*	.....	18.50	22.50	27.2
		Average..	85 173	(50 000)*	(58.7)*	18.83	23.17	27.4
Sheared plates; longitudinal tests	⅛"	10 711	85 300	(50 000)*	.....	23.50	38.00	45.8
	.....	10 711	85 800	(50 000)*	.....	21.50	37.00	45.2
	.....	10 711	82 700	(50 000)*	.....	20.25	37.50	52.0
	.....	10 711	86 620	(50 000)*	.....	22.00	40 00	47.7
	.....	10 711	84 640	(50 000)*	.....	23.25	44.50	51.3
		Average..	85 012	(50 000)*	(58.8)*	22.10	39.40	48.4
Sheared plates; transverse tests	.....	10 711	83 700	(50 000)*	.....	21.00	34.00	35.4
	.....	10 711	84 300	(50 000)*	.....	19.50	35.50	39.6
	.....	10 711	84 380	(50 000)*	.....	24.00	36.00	35.8
	.....	10 711	83 700	(50 000)*	.....	23.75	41.00	46.9
	.....	10 711	83 920	(50 000)*	.....	22.50	38.50	41.2
	.....	10 711	85 960	(50 000)*	.....	19.50	37.00	48.7
		Average..	84 327	(50 000)*	(59.3)*	21.71	37.00	41.3

* Approximate; could not determine accurately.

TABLE No. 3.

Physical Properties of Acid Open-Hearth Forging Steel.

Made by the Pennsylvania Steel Company. Composition, C, 0.25 to 0.30; Mn, 0.60 to 0.80; S, 0.03 to 0.07; P, 0.03 to 0.06; Cu, 0.20 to 0.40.

Shape of member.	Size of member.	Heat number.	Ultimate strength. Pounds per square inch.	Elastic limit. Pounds per square inch.	Elastic ratio. Percentage.	Elongation in 8 ins. Percentage.	Elongation in 4 ins. Percentage.	Reduction of area. Percentage.
Rounds.....	¾" diam.	10 061	75 920	49 000	.....	24.50	.....	58.2
	"	10 020	76 750	53 430	.....	25.00	.....	58.7
	"	10 019	77 240	52 700	.....	25.00	.....	50.3
	"	10 022	77 620	51 680	.....	24.25	.....	46.1
	"	10 034	77 960	52 140	.....	23.25	.....	41.2
	"	10 068	79 120	52 360	.....	22.75	.....	51.9
	"	10 042	79 920	51 400	.....	23.75	.....	56.1
	"	10 047	80 000	51 630	.....	23.00	.....	53.6
		Average..	78 066	51 793	66.3	23.94	.....	52.0
Angles.....	3" x 3" x ⅜"	10 022	75 970	50 020	.....	.....	32.00	48.4
	"	10 019	76 620	50 610	.....	.....	32.50	49.1
	"	10 068	77 040	48 060	.....	23.75	.....	54.9
	"	10 017	77 200	49 580	.....	.....	26.80	47.6
	"	10 020	78 020	49 450	.....	.....	31.25	47.8
		Average..	76 970	49 544	64.4	.....	30.64	49.6
Universal plates. Each figure is the average of two tests.	22" x 1"	8 165	78 230	44 150	.....	22.7	.....	50.6
	36" x 7/16"	10 034	78 635	51 005	.....	26.7	.....	52.0
	36" x 7/16"	10 036	79 005	46 170	...	28.6	.....	54.5
	11" x 1"	10 016	79 030	44 485	.....	26.4	.....	54.3
	36" x ⅝"	10 056	79 460	46 590	.....	32.5	.....	50.9
	11" x 7/16"	10 042	79 615	47 525	.....	23.8	.....	50.5
		Average..	78 996	46 654	59.1	26.78	.....	52.1
Sheared plates. Each figure is the average of two tests.	36" x ⅜"	10 061	78 100	49 400	.....	24.6	.....	55.0
	84" x ⅜"	10 050	78 800	49 070	.....	20.5	.....	40.0
	32" x ⅜"	10 056	78 900	50 800	.....	25.2	.....	51.4
	30" x ⅜"	8 191	79 100	47 600	.....	22.2	.....	54.3
	32" x ⅜"	8 187	79 210	47 100	.....	20.2	.....	49.0
	36" x 7/16"	10 078	79 400	50 800	.....	19.5	.....	54.8
		Average..	78 918	49 128	62.3	22.03	.....	50.8

TABLE No. 4.

Summary of the Averages Given in Tables Nos. 1, 2 and 3.

Shape of member.	Kind of steel.	Ultimate strength. Pounds per square inch.	Elastic limit. Pounds per square inch.	Elastic ratio. Percentage.	Elongation in 8 ins. Percentage.	Elongation in 2 ins. Percentage.	Reduction of area. Percentage.
Rounds.....	Nickel.....	86 015	63 575	73.9	20.19	34.00	46.3
	Hard forging.	87 663	58 055	66.2	16.70	24.44	30.3
	Forging.....	78 066	51 793	66.3	23.94	.....	52.0
Angles.....	Nickel.....	86 960	58 553	67.3	21.75	39.67	50.5
	Hard forging.	87 820	54 153	61.7	19.25	34.83	43.3
	Forging.....	76 970	49 544	64.4	.....	.....	49.6
Universal plates, longitudinal..	Nickel.....	85 773	58 410	68.1	21.08	39.25	52.0
	Hard forging.	82 773	50 163	60.6	20.50	37.67	47.0
	Forging.....	78 996	46 654	59.1	26.78	.....	52.1
Universal plates, transverse....	Nickel.....	86 417	58 203	67.4	16.50	28.92	36.1
	Hard forging.	85 173	(50 000)*	(58.7)*	18.83	23.17	27.4
	Forging.....	.....	.....	.....	.....	.....	.....
Sheared plates, longitudinal..	Nickel.....	85 337	58 169	68.1	19.00	35.50	48.3
	Hard forging.	85 012	(50 000)*	(58.8)*	22.10	39.40	48.4
	Forging.....	78 918	49 128	62.3	22.03	.....	50.8
Sheared plates, transverse....	Nickel.....	84 377	57 260	67.9	17.13	32.50	43.4
	Hard forging.	84 327	(50 000)*	(59.3)*	21.71	37.00	41.3
	Forging.....	.....	.....	.....	.....	.....	.....

* Approximate; could not determine accurately.

TABLE No. 5.

Comparative Miscellaneous Tests on the Steels in Tables Nos. 1, 2 and 3.

Shape.	Kind of steel.	Nature of tests.
Rounds	Nickel	Bent cold, flat upon itself.
	Hard forging	Bent cold, flat upon itself.
	Forging	Bent cold, flat upon itself.
Rounds	Nickel	When nicked, six bars bent an average of 57° before cracking.
	Hard forging	When nicked, five bars bent an average of 54° before cracking.
	Forging	When nicked, six bars bent an average of 35° before cracking.
Rounds	Nickel	When quenched, bent flat upon itself.
	Hard forging	When quenched, bent flat upon itself
	Forging	When quenched, bent flat upon itself.
Angles	Nickel	Bent cold, flat upon itself.
	Hard forging	Bent cold, flat upon itself.
	Forging	Bent cold, flat upon itself.
Angles	Nickel	When quenched, bent flat upon itself.
	Hard forging	When quenched, bent flat upon itself.
	Forging	.....
Angles	Nickel	Opened out flat, without fracture.
	Hard forging	Opened out flat, without fracture.
	Forging	Opened out flat, without fracture.
Angles	Nickel	Closed about 45°, before fracture.
	Hard forging	Closed about 60°, before fracture.
	Forging	Closed shut, without fracture.
Sheared plates, longitudinal	Nickel	Bent cold, flat upon itself.
	Hard forging	Bent cold, flat upon itself.
	Forging	Bent cold, flat upon itself.
Sheared plates, longitudinal	Nickel	When quenched, broke at a very small angle of bend.
	Hard forging	When quenched, broke at a very small angle of bend.
	Forging	.....

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WHAT IS THE LIFE OF AN IRON RAILROAD BRIDGE?

By J. E. GREINER, M. Am. Soc. C. E.

READ AT THE ANNUAL CONVENTION, JUNE, 1895.

WITH DISCUSSION.

Since engineers have recently freely expressed their views on "Some Disputed Points of Bridge Construction," * and are priding themselves upon the strength, firmness and permanency of the structures now being built, it may be well to consider for a few moments the bridges which were built but a few years ago and stand practically side by side with the heavier constructions of to-day, being called upon to do exactly the same work. Many of these bridges were designed to carry the traffic existing at the time they were built, the actual train service having been taken as a basis for the calculation. But the railroad companies have been increasing the weight of their rolling stock step by step as occasion required, and

* See the *Transactions* of the American Society of Civil Engineers, Vol. XXVI, p. 77.

will continue this increase so long as it is more economical to operate a smaller number of heavy trains than a larger number of lighter trains, or until they arrive at the utmost capacity of their bridges. When this limit is reached further increase must cease or the structures must be strengthened or renewed, as their practical life has ended.

What, then, is the life of an iron railroad bridge? If heretofore it has been scarcely 25 years, it was not on account of the structures having failed or worn out under the loads for which they were designed, but because the loads had been increased to such an extent that the structures, generally of a flimsy character, became loose and shaky and totally unfit for the much heavier traffic at greater speed. The following table, showing the increase in weight of locomotives on the Baltimore and Ohio Railroad, indicates in a very forcible manner the

INCREASE IN WEIGHTS OF ENGINES ON BALTIMORE AND OHIO SYSTEM FROM 1835 TO 1895.

Type.	Date.	Weight in tons.
Grasshopper	1835	10.7
Winans' camel, eight wheels	1851	37.3
Perkins, 10 wheels	1863	45.4
Consolidation	1873	52.6
Consolidation	1881	54.3
Mogul	1886	56.6
Consolidation	1887	57.8
Consolidation	1888	62.5
Baldwin, 10 wheels	1890	66.5
Consolidation	1892	67.1
Consolidation	1894	80.4
Electric motor	1895	95.0

Increase in the	20	years	between	1835 and 1855	248	per cent.
" "	10	"	"	1855 and 1865	22	"
" "	10	"	"	1865 and 1875	16	"
" "	10	"	"	1875 and 1885	3	"
" "	10	"	"	1885 and 1895	48	"
Total increase in	60	"	"	1835 and 1895	651	"

Considering the electric motor which will be run over the Baltimore Belt Railroad, the increase in the 10 years between 1885 and 1895 is 75%, or in the 60 years, 788 per cent.

main reason why the lives of iron bridges, on this road at least, have been so very short—they were over-burdened. The other reasons, namely, flimsy construction and poor details, evil influences tending to a short life, are not generally met with in structures built during the past 10 or 15 years, although it must be acknowledged that some rail-

road companies which buy their bridges as so much stock in trade, without employing a competent party to look after their interests, are getting structures but little better than those used a quarter of a century ago and which develop into flimsy affairs just as soon as a little increase in traffic is felt.

Leaving such structures out of the question and considering only those which have been conscientiously designed in accordance with the generally accepted good practice, it may be said that their lives are not to be determined by years, as is the case with animals, nor by tonnage, as is the case with rails, but by the care bestowed upon their maintenance, which care presupposes an intelligent limit being placed upon the concentrated weight of engines and train loads. Having received proper attention, and the train loads having been limited to those for which the structure was designed, and barring unavoidable accidents, there is, apparently, no reason why a good iron or steel bridge should not last indefinitely. But neglect the approaches, allow low joints in the track, use rotten ties, let the metal rust, pay no attention to loose rivets, neglect the counters and laterals, keep the roller bearings clogged with dirt, in other words, place the bridges in the hands of indifferent or incompetent men, and there will be a greater certainty that they will fail before, than that they will be standing after, 25 years.

Of the bridges built during the past 15 years, the author believes he is not far wrong in stating that the greater proportion are carrying loads in excess of their specification requirements. This leads to a very important and serious question, viz., should heavier loads than those for which the bridges were designed be allowed to run regularly, and, if so, to what safe extent can these loads be increased? If it is asserted that train loads cannot be safely increased beyond specification requirements, then experience belies the assertion. If it is acknowledged that these loads can be increased without any possible injury to the structure, it is then implied that there has been a considerable waste of material in the design, which waste cannot be considered good engineering.

It must be admitted that the usual specification stresses can be increased to a limited extent without subjecting the bridges to any immediate danger. It is being done now and has been done in the past, and the structures are still in good condition. The question for con-

sideration, therefore, is not, can working stresses be safely increased, but to what extent can they be increased ?

That there is a limit to all things, the capacity of the best bridges included, goes without saying. That there is a stress which is known to be perfectly safe as well as a stress which, if continued, will undoubtedly cause failure, is also readily admitted. There is, however, a considerable difference between the so-called safe and dangerous stresses. As it is the aim of an engineer to use his bridges so long as they are safe, but to take them down before they fall down, he must determine in his own mind the point where safety ends and danger begins, or, in other words, decide upon the safe capacity of the bridges regardless of the loads which they were designed, with micrometer refinement, to carry. Personal examinations will not always reveal overstrain in the members, as it will rot in timber and wear in rails. It is, therefore, a case of figures, experience and judgment, which appeal at times more forcibly to the conscience of the expert than to the purse-strings of the railroad company, and especially so when the figures indicate danger, while at the same time the bridge may be carrying the traffic to the satisfaction of all except the man who made the figures and who presumably knows what he is talking about.

Now, while some railroad men ignore calculations in connection with bridges under traffic, and from their practical experience, as they term it, are led to believe that a bridge which is apparently sound and has been carrying a certain heavy load has been tested or proved, and can therefore continue to carry this load until signs of failure are apparent, there are others with an equal amount of practical experience and knowledge of the experiences of others beside themselves, who know that while a bridge may be tested by heavy proof loads apparently in a satisfactory manner, it may collapse under a much lighter load without warning or giving any previous indication of failure. A young inspector may find a kinked eye-bar, a split angle, or a post indented by a collision, and honestly believe the structure in a dangerous condition, while it may be perfectly safe ; or the practical railroad man, whose experience has eased his conscience somewhat, may find the same defects in another structure, make a note to the effect that the bridge will be good for another year, and it may collapse upon the passage of the next train. A few figures judiciously considered have undoubtedly prevented unnecessary or untimely

renewal, just as surely as the indifference to what these few figures have indicated in some cases has resulted in a failure. Figures, however, must be tempered with a sound judgment. Each decision should be based upon a positive knowledge as to the stresses, details of design, material, workmanship and maintenance, combined with the necessary experience and ability to decide promptly and correctly, and should not be left to the snap judgment or exaggerated confidence in the personal experience of the county commissioner or practical railroad man, any more than to the immature judgment of the know-it-all theoretical alarmist.

Suppose that the design, material, workmanship and maintenance are of the best, that the structure is in first-class condition so far as can be ascertained by a rigid inspection, but since the bridge was built the rolling loads have been gradually increased until the time comes for the company to ask their engineer if the bridge will carry engines weighing 50% more than it was designed to carry; if not, then what is the heaviest engine that can run regularly over the bridge? These are practical questions and are being continually asked railroad engineers, who must and do answer promptly, although the answers are usually neither discussed publicly nor published, owing to a desire to avoid censure for a transgression of the good practice as to safe working stresses so carefully instilled in students' minds, so faithfully accepted when designing a new structure, and so philosophically stretched out when dealing with an old one. The whole question hinges upon what can be considered safe working stresses, which term has practically lost its former significance, owing to the different practices, since there are some specifications calling for a safe working stress of 9 000 or 10 000 lbs., and others drawn up by engineers of an equal standing give a safe working stress of 14 000 or 15 000 lbs. This large difference is, of course, due to the variable allowances made for impact, and there is practically no difference so far as the strength and weights of bridges are concerned when designed for the same engine loads. It is, however, confusing to the ordinary railroad officer who has neither the time nor inclination to look into the allowances for impact, and who cannot clearly understand why there should be such a great difference in the safe working stresses used by different designers.

Railroad bridges were formerly built with a supposed factor of safety

of five, and there are many railroad men at the present day who still believe that it would take five times the load for which the bridge was designed to break it down, which unfounded belief causes them to have an undue confidence in some very light structures. While it is well known to bridge engineers that there is no meaning whatever in the term "factor of safety," and, in fact, that there is no such a thing in connection with bridge work, there are still some who are loth to give up, and who add certain large and variable percentages to their live load stresses so as to cover the effects of impact, etc., and then assume a uniform safe working stress approaching one-half of the elastic limit of the material. It appears to the author that it is neither rational nor logical to add arbitrary percentages to the live load stresses in the hope of providing for the uncertain and erratic effects produced by a moving train. The static stresses can be approximately determined, but this percentage custom is only a crude guess which, when added to the figured static stress, makes a guess out of what was otherwise a scientific determination. Still another guess is made in assuming the safe working stress and the section of the member finally arrived at is the result of one fact, plus a guess, divided by a guess. In fact the whole method is but a conglomeration of guesses.

There are a few facts concerning material, rolling loads, and contingent effects which are known without resort to guesswork. For instance, it is known that structural material, when repeatedly stressed beyond the elastic limit, will be injured and ultimately broken. It is known that structural material when subjected to loads without shocks will not break at a stress below the elastic limit, no matter how often this load is repeated. The elastic limit of the material is known, also the approximate static stress in each member of the bridge. It is also known that the effects of impact, shocks or blows are more severely felt in the members which receive these blows suddenly and directly and which have little inertia of their own to resist or absorb them. Upon such facts and upon personal experience with bridges under traffic, not with bridges on the drawing board, should be based the foundation of the safe working stresses, which should be made to vary inversely as the known effects of shock vary, or, in other words, they should be a function of the live and dead load stresses without adding any guess to either.

This is the new method of dimensioning quite generally employed,

and the formulas based upon the foundation of facts indicate a rational means of determining sections and providing for dynamic effects, without obliterating stresses which can be calculated. Safe stresses based on the above method mean that a member subjected to a dead load only will be perfectly safe so long as the unit stress does not exceed the elastic limit, that when subjected to a live load it will be safe so long as the unit stress does not exceed one-half the elastic limit, and when subjected to both dead and live loads will be safe at a stress somewhere between that allowed for all-dead loads and all-live loads. The above remarks are admittedly ancient history, but are given here more for the purpose of having a clear understanding as to what is meant by safe working stresses than as an argument in favor of the new method.

If it is admitted that a stress just within the elastic limit is safe for a dead load and one-half this amount is safe for a live load, it must be borne in mind that such high stresses cannot be used in dimensioning bridges, because there are certain contingencies which cannot be readily recognized by any formula, but which must nevertheless receive a proper amount of attention.

First.—Structural material may have defects which cannot be discovered, no matter what care is bestowed upon the inspection and tests. Something must therefore be allowed for such possibilities.

Second.—Something is allowed for bulk, so that members will have a sensible inertia to withstand vibrations and absorb shocks, thereby avoiding flimsy construction.

Third.— It is desired to have bridges somewhat safer than just safe, as super-refinement in this direction is an absurdity.

In addition to the above, a tacit provision is made for future increase in traffic, as train loads have generally gone beyond what was expected at the time of designing. It might also be added that something is thrown in from force of habit.

No one can deny that the above three contingencies are legitimate, and that in order to provide for them a stress less than the elastic limit for dead loads and less than one-half the elastic limit for live loads must be used in considering whether or not a structure is safe or in designing a new bridge. Assuming, then, that the safe working stresses, as accepted in good practice, in addition to the above three contingencies, provide for a future increase in train loads,

and are generous as well from force of habit, it would appear that if the "force of habit" and "future increase" could be eliminated, or, in other words, if it could be determined just how much is allowed for these two items, there would not be much difficulty in fixing upon the utmost capacity of a railroad bridge of good design. Experience with structures which have been working for some years under an increased traffic without injury furnishes the only means of solving the question.

Considering the subject in relation to timber stringers, interesting and valuable data as to the unit stresses successfully allowed in the practice of the principal railroads of the Northwest have been collected and published by Onward Bates, M. Am. Soc. C. E., in his discussion on "Foundations and Floors for Buildings of the World's Columbian Exhibition," a paper read by the late A. Gottlieb, M. Am. Soc. C. E., before the Western Society of Engineers.*

The practices of ten railroads were given, in which one used a working stress between 1 700 and 1 800 lbs., one between 1 600 and 1 700 lbs., two between 1 500 and 1 600 lbs., three between 1 400 and 1 500 lbs., two between 1 200 and 1 300 lbs., one between 1 000 and 1 100 lbs. The above were white pine stringers figured in accordance with the usual practice of making no allowance for continuity over two supports, and when side stringers were used, of considering that they carried the dead loads, and that the main stringers carried the live loads. No impact was allowed. As Mr. Bates aptly says: "If experience is worth anything these figures will speak for themselves."

The author has personally examined many Georgia yellow pine trestles in which the stringers were and are still subjected to stresses of from 1 600 to 1 800 lbs., and has yet to find a single case of cracking or breaking under such stresses, although some of the stringers were eight years old and correspondingly weather beaten. There was one case in which the stresses were as high as 2 400 lbs. for a period of several years, and, while the stringers were very old, there was nothing to indicate that excessive strains had injured them. This was, of course, an isolated case, and is not given here to illustrate common practice, but rather to show what timber, sound and free from injurious knots, has stood in a long service.

Compare successful practice throughout the country with the ridiculously low safe stress of 750 lbs. for white pine and 1 000 lbs. for yellow

* See the *Journal* of the Association of Engineering Societies, Vol. X, p. 577.

pine, laid down as law by text books since the publication of Lanza's experiments, and state why these low stresses should be recommended, when experience has demonstrated that from 80 to 100% increase is perfectly safe. Do the 750 lbs. given in the books consider the future possibility of rot and the necessary decrease in section and strength occasioned thereby? If so, then it is an absurd reason, as no stress can safely be assigned to rotten timber, and it would be much better engineering to use fiber stresses of from 1 200 to 1 500 lbs., and strengthen or renew the stringers upon evidence of injurious rot, rather than to proportion for a stress of 750 lbs., in the belief that rot has been provided for. The actual practice above noted will surely admit of the very conservative conclusion that when stresses are figured from actual wheel loads and so long as the timber is sound, white pine stringers will be safe at 1 600 lbs., and that an occasional stress as high as 1 800 lbs. will do no damage. It is the author's practice to consider 1 600 lbs. for white pine and 1 800 lbs. for Georgia yellow pine as the upper limits, and to renew or strengthen as soon as the condition of the timber becomes suspicious. In designing new work it is very conservative practice to use 1 000 lbs. for white pine and 1 200 lbs. for yellow pine, and in the author's opinion 1 200 and 1 500 lbs. for white and yellow pine respectively, is a safe and economical practice, which will still admit of an increase in engine loads.

During the past few years the author has made many inspections of existing iron bridge structures, and has found cases which he considers as furnishing interesting data for consideration in connection with the utmost capacity or limit of life in railroad structures. Some of these cases are given below and the permissible unit stresses are taken in accordance with the Baltimore and Ohio specifications of 1894, which agree closely with accepted good practice. These unit stresses for wrought iron are as follows:

Tension in bars ..... $7\,500\left(1+\frac{\text{min.}}{\text{max.}}\right)$

" plates and shapes ..... $7\,000\left(1+\frac{\text{min.}}{\text{max.}}\right)$

Compression in chords, posts and main braces. $7\,000\left(1+\frac{\text{min.}}{\text{max.}}\right)-35\frac{}{r}$

Compression in girders ..... $6\,200\left(1+\frac{\text{min.}}{\text{max.}}\right)$

Shearing of rivets 6 000

Bearing of rivets 12 000

In these formulas, l represents the length in inches, and r the radius of gyration.

Case I. I *Beam Girders.*—Span, 26 ft. center to center; three 15-in., 50-lb. I beams under each rail; dead load, 350 lbs.; live load, 3 075 lbs.; total load, 3 425 lbs. per foot on one girder. Permissible unit stress in top flange, 6 830 lbs.; actual, 16 400 lbs.; overstrain, 140 per cent. Permissible unit stress in bottom flange, 8 270 lbs.; actual, 16 400 lbs.; overstrain, 98 per cent. The bridge was in good condition.

Case II. *Deck Plate Girder.*—Span, 20 ft. center to center; one web, 15 x $\frac{1}{2}$ ins., and four angles, 5 x 3$\frac{1}{2}$ x $\frac{5}{8}$ ins.; rivets, $\frac{7}{8}$ in. diameter, with pitch from 4 to 6 ins.; dead load, 300 lbs.; live load, 3 420 lbs.; total, 3 720 lbs. per foot of one girder. Upper flange; safe stress, 6 730 lbs.; actual, 17 280 lbs.; overstrain, 156 per cent. Lower flange; safe, 8 150 lbs.; actual, 19 660 lbs.; overstrain, 141 per cent. Rivet bearing; safe, 5 250 lbs.; actual, 10 540 lbs.; overstrain, 100 per cent. The above girder was figured on the supposition that bending strains were resisted entirely by the flange angles. The bridge was in good condition.

Case III. *Howe Truss.*—Span, 84 ft.; eight panels, depth 16 ft. 3 ins.; first panel rods, two rods 2 ins. diameter, not upset; dead load on rods, 20 500 lbs.; live load, 64 200 lbs.; total, 84 700 lbs. Safe stress, 9 300 lbs.; actual, 17 600 lbs.; overstrain, 89 per cent.

Case IV. *Bollman Bridge.*—Stress on main rods, considering that panel rods do not relieve them; safe, 9 000 lbs.; actual, 22 000 lbs.; overstrain, 144 per cent. Stress on panel rods, considering that they do relieve the stresses in the main rods; safe, 9 000 lbs.; actual, 16 500 lbs.; overstrain, 83 per cent. The over-strained members in this bridge were in good condition, but as it is very uncertain to what extent the panel rods relieve the main rods, the bridge was condemned.

Case V. *Warren Truss of 155-Ft. Span.*—Main ties; safe stress, 10 000 lbs.; actual, 19 200 lbs.; overstrain, 92 per cent. Second brace; safe stress, 6 080 lbs.; actual, 9 690 lbs.; overstrain, 59 per cent. Third brace; safe stress, 2 100 lbs.; actual, 9 320 lbs.; overstrain, 344 per cent. The cross-ties of this bridge rested directly on the bottom chord, producing a transverse stress in addition to the direct stress, some of which stresses were as follows: Safe, 8 000 lbs.; actual, 19 340 lbs.; overstrain, 142 per cent. This bridge was built in 1871. The third braces were 6-in. channels 25$\frac{1}{2}$ ft. long, and had a value of $\frac{l}{r} = 140$,

which would not be permitted at the present day in any first-class structure. The structure was condemned, its life being about 20 years.

The foregoing five cases of overstrained bridges cover pretty well the ordinary conditions as to style, and were selected especially for the purpose of embracing the field of beam girders, plate girders and truss bridges. They are not isolated cases, and many more illustrations of the same kind could be brought forth if necessary. Before drawing any conclusions, however, it may be well to mention that the author has seen the vertical end posts of a deck Whipple truss, composed of two 8-in. channels weighing 12½ lbs., latticed by 2 x ¼-in. bars, flat at one end and hinged bearing at the other, with a total length of 33 ft. 4 ins., and a value of $\frac{l}{r} = 132$, bend sideways 1½ ins. when overstrained 52%, when figured according to the Baltimore and Ohio formula for compression members, or overstrained but about 8% when figured by Rankin's formula for columns flat at one end and hinged at the other. The posts were connected in pairs and both bent in the same direction. They carried the load on one panel only.

The author has also known short floor beam hangers which were bent around a pin in the form of a yoke, to snap square across through both arms at a strain of 12 930 lbs. per square inch, or at an overstrain of 80 per cent. This, however, was an isolated case and was undoubtedly due to a very poor quality of iron, as the breaks occurred in the body where there were 6 sq. ins., while there were less than 5 sq. ins. through the thread.

Main rods in Bollman trusses have snapped short off without reduction of area or elongation, under a stress of from 25 000 to 27 000 lbs., the panel rods not considered as carrying any of the load, or at an overstrain of from 138 to 157 per cent. As it is almost certain, however, that the panel rods did carry a part of the load, this overstrain would be considerably reduced. Again, while the author has never yet met with an I-beam girder which failed in service, either by buckling of the top flange or tearing apart of the lower flange, he has seen many cases where the webs have split open for a distance of 3 or 4 ft. when the train loads were but 50% in excess of what the beams were intended to carry.

No conclusions could be drawn from the particular cases cited had they been isolated, but as stated before they are not isolated and were

selected for illustrations of numerous similar conditions found in practice.

One thing is pretty clearly brought out, and this is that if the limit of elasticity of wrought iron is considered as 26 000 lbs., and an all-moving load is held to produce twice the strain that is caused by an all-dead load, then something is radically wrong in the usual method of figuring stresses from concentrated wheel loads; otherwise, each of the structures mentioned should have shown a permanent set, which they did not. If the wheel method of figuring stresses is correct, then the preceding examples demonstrate without question that an all-moving load does not produce twice the stress caused by an all-dead load. It is positively certain that the method of figuring stresses from concentrated wheel loads causes strains so far above what really exist as to almost class this method as unscientific, because there can be no concentrated loads when there are stiff rails and cross-ties spaced at close intervals placed between the load and the supporting girder. A distributed load equivalent to the sum of the concentrated wheel loads of the engine would give stresses nearer the truth. While it has been, and is generally, considered that a suddenly applied load produces about twice the stress of the same load at rest, and that an engine moving over a bridge is a suddenly applied load, there is considerable doubt in the minds of practical bridge men as to the truth of this assumption. In fact bridge men who are continually looking after bridges in service are strongly inclined to believe that, so far as strains are concerned, the mere fact of a train rushing across a small opening does not in itself cause any greater strains than should the same engine cross slowly, as it takes time for strains to be felt and transferred through the body of the supporting girders. A rushing engine gets across in so short a time as to probably eliminate all impact. Low joints in the track and unbalanced drivers do, beyond all question, cause blows to be delivered to the supporting girders, and the greater the speed, the more force there is in these blows, and considered in connection with unbalanced drivers, low joints and rotten ties, great speed does cause impact. But with the track in proper condition and fairly well-balanced drivers, the author believes that the quicker a train gets across a weak bridge of very small span, the better it will be for the bridge. This of course does not apply to truss bridges, long trestles or viaducts.

As bridge engineers in practice who are frequently called upon to answer the question suggested in the subject of this discussion must base their judgment largely upon what has been done and should be somewhat consistent in their opinions, it may be well to arrive at some understanding as to what can be considered an upper limit in stresses produced by the constantly increasing engine loads. When these limits are reached it is time to strengthen or renew, and it must be distinctly understood that the suggestions advanced by the author apply only when stresses have been determined from actual wheel loads, when the structures are of good design and in good condition, and where details have been made to develop the full strength of the members. Details of connections have heretofore been the weakest parts of bridges, are the first to cause trouble, and, as a rule, must be strengthened long before the main sections are in any danger.

While no engineer would for an instant think of using the stresses suggested below in making new designs, it is necessary at times to use them in old work, and while it is believed that these stresses will be safe for a long period, there is no certainty as to how long. Whenever permitted, the structure should be under very careful supervision. There is no doubt that modern steel bridges are practically permanent so long as they are properly maintained and so long as the loads do not exceed the specified requirements, and if there was any assurance that they would be equally permanent under an increased loading of 50 or 75%, the fact would soon be recognized by increasing the present accepted unit stresses and adopting lighter structures.

The following unit stresses are suggested as the highest permissible for a constant service. An occasional load a little in excess of these limits will do no harm.

a. Timber beams free from loose or rotten knots and in sound condition. Unit stress on extreme fibers; white pine, 1 600 lbs.; long leaf yellow pine, 1 800 lbs.

b. Rolled iron beams, braced firmly together, in groups of two or three, so as to form a compound girder, the unsupported lengths of compression flange not exceeding 16 times its width. Unit stress, $13\,000 \left(1 + \frac{\text{min.}}{\text{max.}}\right)$.

As the webs are likely to split open at a less stress than indicated above, beam girders should be closely watched whenever stresses exceed

the specification requirements by 40 per cent. These cracks, however, can be readily prevented by means of stiffening plates extending from flange to flange and riveted, both to the flanges and to the web at each end of the beam. Cracked beams will still do good service when the cracks are strongly spliced.

c. Plate girders with an efficient system of lateral bracing, stiffened web, rivets correctly proportioned, cross-ties resting on the upper flange or on an iron floor system. Main girders, compression flange, $11\,000\left(1+\frac{\text{min.}}{\text{max.}}\right)$; main girders, tension flange, $13\,000\times\left(1+\frac{\text{min.}}{\text{max.}}\right)$.

All connections of stringers to floor beams and floor beams to girders in through bridges should be strengthened when the shear exceeds 9 000 lbs. on the diameter of rivets, or the bearing exceeds 18 000 lbs., and all loose rivets should be replaced by tight ones as soon as discovered. When cross-ties rest directly on the lower flanges, an increase in flange stress of 50% would be uncertain and liable to cause trouble.

d. Truss bridges.—In Bollman bridges the main rods will be safe at a stress of 20 000 lbs. when the panel rods are in good adjustment and are capable of aiding the main rods. When the panel rods are loose or cannot be adjusted, owing to rusty threads or other causes, the stress should not exceed 16 000 lbs. per square inch.

In Pratt or Warren bridges, the stresses in the main ties, lower chords, long suspenders or counters should not exceed $13\,000\times\left(1+\frac{\text{min.}}{\text{max.}}\right)$. Stresses in short bar hangers should not exceed 12 000 lbs. Upper chords and posts, when the ratio of the length to the least radius of gyration does not exceed 115 or 120, can have the stresses increased to $12\,259\left(1+\frac{\text{min.}}{\text{max.}}\right)-.61\frac{l}{r}$.

If the above conclusions are accepted, then the life of an iron bridge of moderate span will have reached its limit when train loads have been increased between 80% and 90% beyond what the bridge was designed to carry. They must not be construed to mean more than they affirm, and when used in actual practice it must be remembered that there are other parts of a bridge besides the ties and columns, and that while these main members may be reasonably safe, the structure, depending, as it does, upon other features as well as its main members, may be treacherous.

DISCUSSION.

Mr. Morison. GEORGE S. MORISON, President Am. Soc. C. E.—The conditions which the author has brought out, namely, that a bridge will do good service under strains which are very much greater than it was intended to bear, show what is now one of the greatest defects in bridge specifications. It will not do to say that no bridge has ever failed because the metal in a main member was overstrained, but this is an extremely rare occurrence; the difficulties, wear and everything else, occur in the details. Bridges are generally designed for a certain strength and proportioned so that their metal shall not be overstrained, but there is scarcely a specification drawn which limits the distortion which a bridge may be permitted to undergo. Yet it is perfectly possible to design a bridge in which the metal of every member would be strained up to 20 000 lbs. to the square inch, and have a bridge which would wear better, last longer, and be a better structure than another one in which the strains are limited to half this amount, even though the second structure be proportioned properly in all its details. A difficulty in many bridges is that the trusses, as well as the different members, can change their shape too much without passing proper limits of strength, and this is a point which has scarcely ever been provided for in specifications. An examination of an elevated railroad structure, which was actually erected and in which everything was properly proportioned so far as strain was concerned, showed that the structure, without overstraining any metal, could move 9 ins. out of line horizontally; nobody would think this a proper condition to allow. This is a point which has not been given proper attention, although there are many things about specifications which go into minutiæ.

The matter of counter-balancing has been referred to. Many locomotives, which theoretically have 15 000 lbs. on an axle, really when moving at high speed exert pressures varying from 32 000 to minus 2 000 lbs., which is rather an unfortunate condition; but still these engines are running all the time. If a structure is so designed that it keeps its shape under the passage of a train, or if it changes its shape in so gradual a way that the change is not felt, it may be all right even with very high strains.

In an important bridge an examination was made and the pins were found to have a theoretical bending strain in extreme fibers of from 175 000 to 250 000 lbs., and yet when some of those pins were taken out they were perfectly straight. Unfortunately no observations were taken to show the actual variations of strain in the bars attaching to the pins, but if the bars had been examined under strain with a micrometer and the strains in them had been found to differ not more than 15% from a mean, the bridge should have been accepted as a safe

structure, even though the theoretical bending strains in the pins exceeded 300 000 lbs. to the square inch. It was simply a case where the theoretical strains in the pins are of no real importance, provided the pins stay straight; in other words, they do not exist if the pins are straight. Mr. Morison.

A great many of the older bridges were designed with very high strains. Albert Fink, Past-President Am. Soc. C. E., 25 years ago considered 14 000 lbs. to the inch a perfectly proper strain to put on the tension members of his bottom chords, and none of his bridges proportioned in this way failed to give good results, though most of them have now been superseded by structures adapted to very much heavier rolling stock.

T. C. Clarke, M. Am. Soc. C. E.—Among the most remarkable instances in the United States of bridges built to carry light loads, which now support trains much heavier than those for which they were designed, are the riveted lattice bridges constructed on the New York Central Railroad about 1869 or 1870. For 25 years they have stood with but little reinforcement. There have been a number of collisions on them, but in no instance, so far as the speaker knew, had one of them yielded or broken. This is a very remarkable record, in view of the great increase of rolling loads. The speaker had watched the passage of trains over some of these bridges and attributed their great strength and durability to the fact that the strains are diffused instead of being concentrated. The practice of the designers of these bridges was to place the diagonals very close together as compared with the modern long panels. This led to diffusing the strains over a large number of points rather than concentrating them on a few, and this is one of the causes which make the bridges stand so well. This form of design, followed to its logical conclusion, leads to plate girders where a continuous web is attached to the chords by rivets very close together. Experience shows that the durability of plate girders is greater than that of any other form of construction. Mr. Clarke.

Joseph M. Wilson, M. Am. Soc. C. E.—The speaker did not believe that iron bridges give way materially from deterioration or wearing out, but considered that the troubles are occasioned by overloading or by defective design or construction. There were bridges constructed on the Pittsburg Division of the Pennsylvania Railroad, when it was first built, which were loaded above the original estimates at least 50% for 15 to 20 years, but they withstood the extra service without any difficulty. They were subsequently doubled up and used for a number of years, and finally stone bridges were put in their place. Material taken from these old bridges after 20 years of service was found on testing to have suffered no deterioration of any nature. During the civil war there was frequently occasion to estimate the increased loads which the Pennsylvania system of bridges would sustain safely, Mr. Wilson.

Mr. Wilson. and the speaker often had no hesitation in allowing an overloading of 50 per cent. There were many plate girder bridges of various spans, with depths of 5 to 5½ ft. They were built of very thin material, the webs being only $\frac{3}{16}$ in. thick, and the verticals of cast-iron, held by three bolts, acted merely as separators between the upper and lower flanges. Although these bridges rendered good service for many years, they could not have withstood rigid scrutiny under the present rules for calculating such structures.

CORRESPONDENCE.

Mr. Stanwood. JAMES H. STANWOOD, M. Am. Soc. C. E.—Near the end of the paper the following statement appears: "There is no doubt that modern steel bridges are practically permanent so long as they are properly maintained * * * " It is the object of the following notes to emphasize by mentioning some actual cases of neglect the importance of protecting iron and steel structures against rust.

Many articles have been written in recent years pointing out in no uncertain manner the grave dangers to which metallic structures are subjected by corrosion, yet instances are not lacking where authors have expressed the opinion that these dangers are greatly overestimated and that they do not call for serious consideration.

The well-known experiments of Mr. Mallet, of England, made as early as 1840, show that good iron will lose in thickness in 100 years, 0.215 in. in clear sea water, 0.404 in. in foul sea water, and 0.035 ins. in clear fresh water.

While it is held by some that sea water is a very active corrosive agent, others have observed that iron rusts no faster in clear sea water than in the air of a smoky city. Foul water is one of the most active agents of corrosion, as the writer has observed in the case of certain beams, etc., used in connection with drainage works. Drippings of brine from refrigerator cars have caused much trouble, rusting rapidly parts of the bridges over which they pass.

Smoke from locomotives, causing serious rusting of overhead bridges, roofs of round-houses, train-sheds, etc., is one of the most important elements against which the bridge engineer has to contend. Just what is the cause of the corrosion is not fully agreed upon. Many (including the writer, who has found sulphate by chemical tests in rust caused by smoke fumes) believe that the sulphur in the coal is changed by combustion to sulphurous acid, which is transformed in the presence of exhaust steam to sulphuric acid, which, though weak, certainly does have a very destructive effect, especially if confined more or less and allowed to remain in contact with iron or steel.

Others consider that the immense volume of carbonic acid gas emitted by the locomotives is the active agent of corrosion. Whatever the cause, the fact remains that locomotive fumes are a serious menace to the life of iron or steel structures. Mr. Stanwood.

The following extreme instances have recently come under the writer's notice and may be of interest:

In 1867 the late E. S. Philbrick, M. Am. Soc. C. E., constructed a plate girder highway bridge in Boston, Mass., carrying a street over an important railroad. The bridge crossed a part of the yard including certain tracks on which a large amount of shifting was done, consequently the under part of the bridge was seldom free from smoke fumes, since the floor was so made and the location was such that there was little chance for the smoke to clear. The head-room was a little more than 14 ft. When the bridge was erected it was painted with zinc paint covered with two coats of mineral paint. So far as is known this bridge was not painted for 20 years, when it was thoroughly examined and cleaned, and a coat of red lead applied to the corroded portion. At that time the bridge was so badly rusted in spots that its safety was considered doubtful. In 1889 it was removed and the writer had an opportunity to obtain several pieces, illustrations of which are presented in Plate V.

Fig. 1 represents a portion of the lower flange of a main girder at the abutment. Fig. 2 is an end view of the same specimen. The angle irons were originally $3\frac{1}{2} \times 3\frac{1}{2} \times \frac{7}{8}$ ins., with an area of about 2.48 sq. ins. each. The area of the most corroded angle is 1.33 sq. ins., showing a maximum loss in 22 years of 46.4%, an average of 2.1% per year. The area of both angles at the same section after corrosion was 3 sq. ins., a loss in 22 years of 39.5%, or an average of 1.8% per year. Fig. 3 represents a section of the web of the main girder, showing a perforation by rusting. This web was originally $\frac{7}{16}$ in. thick. Fig. 4 shows a batten which was exposed to the fumes all these years; originally as thick as the web, it is now nearly perforated. The protection afforded by the rivet heads can be clearly seen.

These examples may be considered as extreme cases of rusting, for probably no bridge is ever likely to be more abused than this one was by neglect.

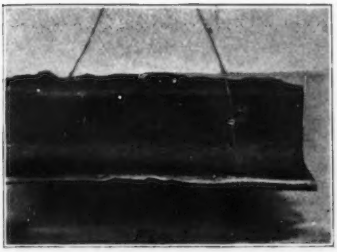
A few hundred feet from this bridge was another crossing the same railroad. It was a Boles patent bridge of a complex construction, built in 1864–65 and removed in 1890. Fig. 5 represents a portion of a cable of seven strands, each strand composed of seven wires of No. 6 Am. W. G. This cable, which was not in the original patent, was provided with screw adjustments and was doubtless added to strengthen the lower chord. It was entirely eaten away as shown, at a point over one track, in certainly less than 25 years. Fig. 6 shows the end connection of a truss rod for a wooden floor beam. The

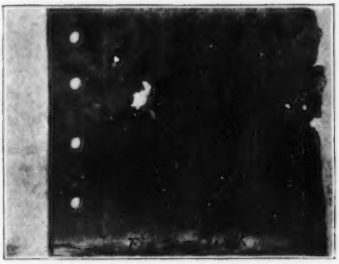
Mr. Stanwood. upright piece in the view was originally $1\frac{3}{4} \times \frac{5}{16}$ ins., or 0.55 sq. in. in area, and now its area is 0.26 sq. in., or a loss of 52.7% in 25 years, an average of 2.1% per year. Fig. 7 represents two hanger bolts originally 1 in. in diameter. The present area at the smallest diameter is 0.37 sq. in., a loss of 52.2% in 25 years, an average of 2.1% per year. Fig. 8 is a view of still another bridge in the same neighborhood, existing under similar conditions. The large hole in the web can be seen.

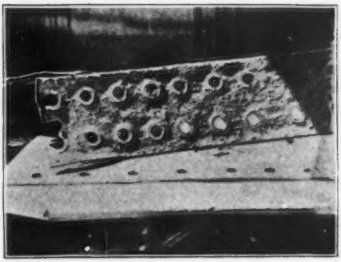
The above illustrations may be a better argument than words in favor of careful and frequent inspection, and the application of the best preservative materials to exposed iron and steel in engineering structures. It will not be disputed that a large part of the lives of the bridges mentioned in connection with the illustrations was wasted by neglect.

Mr. Bates. ONWARD BATES, M. Am. Soc. C. E.—The life of an iron bridge will be definitely known when some of them have worn out under the conditions of usage for which they were designed. The writer does not know of any iron bridges which have failed under such usage, except such as would be thrown out of this discussion on account of faulty design or workmanship. The author's statement that iron bridges properly cared for and not overloaded should last indefinitely is warranted by the lack of contrary evidence, and since a limit to the life of many iron bridges has been found, he naturally looks to the cause of the limitation, and finding it to consist in excessive loading, he draws the logical conclusion that the life of a bridge is to be measured by the amount of increase in loads. Engineers who have charge of the maintenance of railway bridges can appreciate that conclusion, for the increase in weight of rolling stock constantly brings up questions of the strength of existing bridges.

These questions will recur so long as rolling stock is designed with no other consideration than to obtain the maximum pulling power of engines and carrying capacity of cars. In determining the probable ultimate strength of an iron bridge, the author's conclusion that loads may be increased to 80% or 90% beyond what the bridge was designed to carry is well enough for the net sections of the principal members; and he properly adds, in the same sentence, and by way of conclusion, "that while these main members may be reasonably safe, the structure, depending as it does upon other features as well as its main members, may be treacherous." It is these "other features" which cause the engineer of maintenance to have uneasy dreams, and as each of them is a problem which requires separate treatment, no general formula can be given for the life of an iron bridge. The problem that engineers are most interested in is to obtain the longest service from bridges, and the writer ventures the following remarks, giving some of the causes which tend to shorten the life of a bridge, and some of the means which may be taken to prolong it.







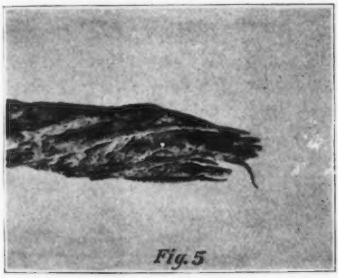


Fig. 5

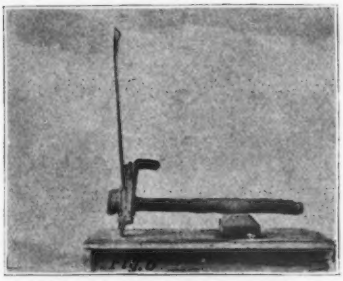


Fig. 6

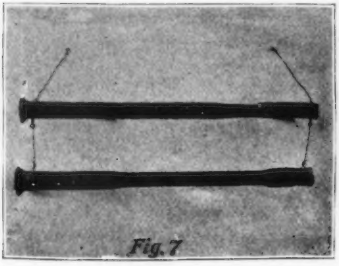


Fig. 7

Fig. 8

Mr. Bates.

The excessive loads to be provided for may be classed as regular and unusual. Regular loads are heavy locomotives and loaded cars in the regular service over the line upon which the bridge is located Unusual loads are experimental locomotives, locomotives in transit as freight, rotary snow plows, unusually heavy loads of boilers, machinery, stone, etc., and bad loading of cars, throwing excessive loads upon the trucks. It is not the weight of a train which puts the maximum stress upon the main members of a bridge that is to be feared, but rather the local loads which bring the maximum stresses upon the "other features." What is probably one of the worst cases of local loading is a locomotive hauled "dead," with driving wheels uncoupled and hammering the track at every revolution. The "other features" in bridges which are to be watched can scarcely be enumerated, for they are to be summed up in the phrase "details and connections."

The statement that the best design is the one which gives the greatest strength and durability will apply with full force to bridges which are to be built, but only to a limited extent to existing bridges. With bridge iron at $40 per ton, there is no excuse for building bridges in which there will be loose rivets or other weak spots, under even excessive service. Unfortunately the proportions of existing bridges stand in the way of adding material *ad libitum*, but in many cases it is possible to strengthen the weak portions of an existing bridge and greatly increase its life. How to do this can only be determined by a study of the bridge in question. A bridge may be strengthened while in service by reinforcing its members, by adding members, and by cushioning those members which are subject to sudden loading. To illustrate the latter method of strengthening, the writer may instance the application of heavy fir stringers of two panel lengths and breaking joints over the floor-beams of some light truss bridges. These timber floors absorb impact effects, as well as distribute the rolling load, and the comparative smoothness of riding over them is very noticeable. The cushioning effect of timber floors and wall plates is very beneficial, and these are often worth more than the cost simply as cushions.

Since the author has drawn some deductions from a statement by the writer, giving working stresses in pine stringers, the latter thinks proper to take exception to the former's statement, that in fixing the section of pine stringers it is absurd to consider and provide for the future possibility of decay. On the contrary, he deems it more reasonable to provide for the decay which is sure to occur than to put an excess of section in an iron bridge to provide for a future load which is only a probability. Timber stringers cannot be renewed with economy as soon as they begin to deteriorate. The author's figures, "unit stress on extreme fibers; white pine, 1 600 lbs.; long leaf yellow pine, 1 800 lbs.," are safe for "timber beams free from loose or rotten knots and in sound condition." But it must be remembered

Mr. Bates. that timber is not made by a mechanical or chemical process, that it is a natural product and must be taken as it grows, and in proportioning bridge stringers it is well to consider the primary defects of the material, such as knots, shakes and sap, and the defects occurring in service, such as season checks and decay, and provide an excess of material to compensate for deficiencies.

Mr. Purdon. C. D. PURDON, M. Am. Soc. C. E.—The title of the paper might read, "How much will iron bridges carry safely over and above the loads they are supposed to be designed for?" as this is what the author investigates, and his conclusion would seem to be that when the loads have reached above what might be carried safely permanently, it is time to renew the bridge. The object of the paper is thus to ascertain what strain may be safely carried by the material used in the bridge, and also, what strains a certain weight of engine will produce. The latter part seems to be the more important, as it is recognized that an engine moving over a bridge will produce strains much in excess of what would be produced by the same engine at rest.

The author says that there is considerable doubt in the minds of practical bridge men as to the assumption that a suddenly applied load produces about twice the stress that the same load would produce at rest, but some good authorities are satisfied that it is so, nevertheless. A paper by George S. Morison, M. Am. Soc. C. E., which the writer has not at hand, but quotes from memory, concludes that the manner of balancing the driver of an engine can, at high speeds, cause twice the stress it would at low speeds. C. C. Schneider. M. Am. Soc. C. E., provides in a late specification a graduated table of impact to be added, running almost as high as 100% on short spans, and S. W. Robinson, M. Am. Soc. C. E., in his report to the Ohio Railroad Commissioners, 1884–85, comes to the conclusion that all spans should have not less than 50% added for impact, unless the truss were so designed as to avoid vibration.

The writer knows of some floor beams of white pine which have carried 1 600 lbs. stress for years and which are still in good condition, and 1 250 lbs. is often used for trestle stringers, but J B. Johnson, M. Am. Soc. C. E., in his tests for the United States Government found a variation of nearly 100% in the sticks cut from the same tree.

The load proposed for plate bridges is $13\,000\left(1 + \frac{\text{min.}}{\text{max.}}\right)$, and taking the example he gives of an old girder (page 303) this expression would give $13\,000\,(1 + \frac{3\,0\,0}{5\,7\,2\,6}) = 14\,040$ lbs. for the allowable strain. This bridge was already carrying 19 650 lbs. and was in good condition. Professor Robinson in his report above quoted considers that 15 000 lbs. could be used. If all of the strains could be accounted for, the unit strain could approach very much closer the elastic limit of the material than now allowed.

The writer has lately investigated the effect of a rolling load 50% above that specified. In the end post, to increase the load 50% and allow nothing for impact would cause a stress of 8 600 lbs. per square inch, while to make the same increase and add 100% to the increased load for impact would cause a strain of 15 460 lbs. per square inch on a column which has an ultimate strength of 35 486 lbs., according to Osborne's tables. On tension members of the web, adding 50% to the moving load and neglecting impact gives 13 715 lbs. per square inch, and adding 100% to this increased load for impact gives 24 930 lbs. per square inch. On the bottom chord, adding 50% as before, gives 13 945 lbs. per square inch., and adding 100% to this gives 25 000 lbs. per square inch. For the bottom flange of floor beams, adding 50% gives 15 000 lbs. per square inch, and adding 100% for impact gives 27 900 lbs. per square inch. The material is soft and medium steel with an elastic limit of 30 000 and 35 000 lbs. Mr. Purdon.

An examination of the New York Railroad Commissioners' report on bridge strains, published in 1891, shows that Howe truss rods carried 14 000 lbs., lattice girders had angles under a tension of 10 300 lbs., and trusses had tension members of the web at 13 000 lbs., and counters as high as 15 000 lbs. per square inch. In one or two cases, counters ran as high as 30 000 lbs. per square inch, but these must have been slack.

The author confines himself to bridges lately built, but it seems to the writer that the question, "Will the bridge carry engines 50% heavier than it was designed for?" is more likely to be asked about old than new bridges, at least of the present generation of bridge engineers. It has been the writer's experience that in old bridges when calculated for present loads, the trusses show up fairly well excepting the hip vertical, and the weakest parts are the floor and its connections, so that each bridge would have to be examined for its weakest point before the question could be answered, and as the weakest point governs the bridge, so does the weakest bridge govern the division. Almost any road, except a very new one, is likely to have bridges for many different loads, and the question of what engines can be used will depend more on the old bridges than on the new ones. Figures alone will not be sufficient, for the physical condition of the bridge should be ascertained in addition to knowing what the shear and bearing per rivet is in a connection. The condition of the rivets should also be known.

G. B. Hazlehurst, M. Am. Soc. C. E.—The paper brings up a subject which caused the writer many hours of anxious thought when in charge of a large number of old bridges, most of them seriously overstrained. The question as to what limit of strain should be allowed in different members before condemning the structure is one that has received a great deal of attention on his part. In connection with the late James L. Randolph, M. Am. Soc. C. E., it was finally de- Mr. Hazlehurst

Mr. Hazlehurst

termined that the admissible increase in strain in a bridge that was otherwise in good condition might be placed at 50% above the unit strain usually considered as allowable with the ordinary factor of safety of five; or to place it more plainly, assuming the safe strain to be 10 000 lbs. per square inch for tension members, the limit was fixed at 15 000 lbs. per square inch, up to which point a member was allowed to do its daily duty without either strengthening or condemning the structure as too old. For compression members, the allowable strain per square inch usually given by the ordinary compression formulas, with an increase of 50%, was the limit fixed upon. After these strains were reached, a structure was usually trestled or strengthened temporarily if possible, awaiting its turn for replacement. Under this system, the writer carried many old bridges for several years without a single accident of any kind and with entire safety, proving to his own mind that the limit fixed was well within the limit of safety and of fairness to his company.

A nice point is before an engineer when he decides such a question; he does not wish to run any risks as regards destruction of property or loss of life, and at the same time it is important that his company should be put to no unnecessary expense. Observation of a structure under load usually influences an engineer in diagnosing the case before him, quite as much as the general appearance of a patient influences the treatment prescribed by a doctor. The writer has seen bridges show elasticity and stiffness under loads straining the tension members as high as 18 000 to 20 000 lbs. per square inch and the compression members in about the same proportion, while other bridges of inferior design would not show as well with a stress 50% less; consequently, observation is quite important in addition to an analytical determination of the stress in a structure. The writer is of the opinion that engineers to-day become a little timid as soon as the usual factor of safety is exceeded. He has known bridges to carry a heavy traffic for many years with entire safety, with a unit stress that would horrify a man not familiar with, or one who was not accustomed to watch, the behavior of structures under heavy loads.

With reference to the point brought up in the paper in regard to deflection of bridges under static loading and under rapidly moving loads, the experience of the writer agrees with the author's views. Some years ago, the late James L. Randolph, M. Am. Soc. C. E., experimented with a span of about 100 ft. length, placing on the structure an engine and train load, and observing the deflection while standing. The train was then run across the bridge at varying speeds, and at a speed of about 30 miles an hour the deflection noted was only about one-half of that shown when standing. The writer has not the exact data to refer to in the case, but the results were substantially as stated, and the probable reason for them was that it takes time for the

strains to be diffused through a structure; a train rapidly rushing across a bridge does not really give time for all the members to be sufficiently affected simultaneously to produce a maximum result. Mr. Hazlehurst

M. E. YEATMAN, M. Am. Soc. C. E.—Some remarks giving for comparison the experience and practice of the railroad with which the writer is connected may be of interest as bearing out in the main the conclusions in the paper. The weight of the locomotives of the Norfolk and Western Railroad Company has increased during the last 20 years in a very similar ratio to that on the Baltimore and Ohio system. This increase, and not any actual defect or wearing out, has limited the life of the iron bridges. Mr. Yeatman.

The oldest bridge still in service as originally constructed was built in 1878. The strains in this bridge are about up to the assumed limits of safety as stated below, but the structure, which is a Warren truss with all the members, tension as well as compression, of rigid form, is remarkably stiff and free from vibration, and shows no signs of weakness. Another bridge of the same date, a deck Warren truss, has had the top chords reinforced on account of the strains from bending and compression combined passing the limit laid down, otherwise the trusses are in service as built.

Bridges built in 1880 were designed for consolidation engines with 20 000 lbs. on each axle, and are still in service, though not allowing much margin for any further increase of rolling loads.

Bridges built in 1884 were designed for loads of 24 000 lbs. per axle; those built in 1889, for 30 000 lbs. per axle; those in 1891 and since, for 34 000 lbs. per axle, or 40 000 lbs. on each of two axles, this last specification giving a margin of from 6% to 10% over the heaviest engines in actual use.

The iron bridges of earlier date than 1878 in service on this system have been strengthened in all cases, or doubled up by putting three or four trusses in place of two to a track, or else are supported by trestles at two or more panel points. The general rule adopted for a guide in this matter has been to strengthen or replace bridges whenever the strains on truss members, under the heaviest engines, exceed by more than 50% those allowed by specifications; plate girders, when they exceed 15 000 lbs. per square inch on net section in tension; and stringers (yellow pine), when they exceed 2 000 lbs. per square inch. Of course room is allowed for judgment. Individual cases and several plate girder bridges showing less than the above strains have been trestled on account of visible permanent deflection, due, probably, to defective work in the riveting.

On pins, as calculated for bending, and on the bearing area of rivets, somewhat more latitude has been allowed, as is justified by the observed facts on a bridge in Pennsylvania (since replaced), which carried for years without visible ill effect loads producing a calculated

Mr. Yeatman. pressure on the bearing area of rivets in webs of floor beams ⅞ in. thick of 36 000 to 40 000 lbs. per square inch, and a strain equal to 82 000 lbs. per square inch when calculated in the regular way. It is hardly to be supposed that this strain was actually borne by the pins, but it could only have been avoided by a very unequal distribution of strain between the chord eye-bars, or over the surface of the eye-bar pin holes.

Mr. Le Conte. L. J. LE CONTE, M. Am. Soc. C. E.—The life of a railroad bridge depends upon how rapidly the future weights of engines and train loads are increased over and above those assumed at the time of its construction. This is a problem which is becoming more and more important each year. It is a serious question for the engineer to face, for the reason that this increase in the live load is a matter which is largely beyond his control. The author calls attention to the surplus iron in existing bridges, as proved by the great increase in engine and train loads without serious results, and comes to the conclusion that there has been a waste of material in the original design. The inference to be drawn is that there is too wide a margin between safe stresses and dangerous stresses. In the case of heavy bridges such as the Forth Bridge, the dead load is the only factor used in determining the size of members, the live load effect being a nonentity. Here the engineer can estimate with considerable certainty the point where dangerous stresses begin. On the contrary, in the case of light bridges the live load is the controlling feature in fixing the safe stress unit, and here is just where the great discordance in practice comes in. The man is not born yet who can deduce a definite expression which will give even a rough approximation for determining the relation between the live load effect and its equivalent dead load. All the useful knowledge on this subject, worthy of the name, is that obtained from actual experience and observation on existing bridges. Of course the results obtained are more or less conflicting, but it is the best and only trustworthy information. By the nature of things this knowledge is purely empirical, and empirical formulas are notoriously dangerous tools to play with. Hence it is practically impossible to say, in the case of light bridges, where the point of safety ends and danger begins; there is necessarily a wide and uncertain interval. In such cases the unit stress adopted must be nothing more than an intelligent guess. The author's reflections are quite right when directed against present practice as applied to heavy bridges, but the writer is not at all prepared to admit the fairness of his remarks when applied to safe stresses for light bridges.

Mr. Katté. WALTER KATTÉ, M. Am. Soc. C. E.—As types of the long span lattice bridges on the New York Central Railroad, mentioned by Mr. Clarke, the writer has selected three representative spans. They were built before his connection with the road, and the following information was obtained from the records of the office.

Mr Katté.

Bridge No. 381, Mohawk Division, over Erie Canal at Schenectady. This bridge is a multiple intersection lattice riveted truss of 155 ft. clear span; depth of truss, 20 ft. It is divided into 23 panels of 6 ft. 8½ ins. each. The floor system consists of 9-in. I beams about 2½ ft. apart, suspended from the truss bottom cords by four rivets of 1 in. diameter, in direct tension. It was built in 1866 and was strengthened in 1888 at a cost of $1 160 for labor and materials.

Bridge No. 578, Mohawk Division, over the Erie Canal at Canastota. The spans of this bridge carrying the two freight tracks Nos. 3 and 4 were built in 1864 and re-inforced in 1889. This span is 128 ft. in length with 20 ft. depth of truss. It consists of one center and two outside trusses, each containing 11 panels of 10 ft. each, and two of 8 ft. 11 ins. each. The greatest stress per square inch, as reported in the Railroad Commissioners' report of 1891 on bridge strains, is 11 300 lbs. per square inch.

Bridge No. 597, Western Division, over Erie Canal at Syracuse. This is a multiple intersection riveted lattice span, 130 ft. in total length, and 118 ft. clear span, with 20 ft. depth of truss. It carries three tracks supported by four trusses. The tracks are carried on transverse I beams which are suspended from the trusses by four rivets attached to each truss, the load being suspended by direct tension on the heads of the rivets. It was built in 1861 and was never strengthened until this year. It is now undergoing a process of re-inforcement. The stresses in certain members before strengthening were as high as 16 000 lbs. per square inch, yet it never exhibited any serious signs of weakness. The floor system is typical of bridges built at that date on the New York Central road, and would no doubt, if designed at the present day, call out considerable adverse criticism; nevertheless it has stood the test of 34 years of hard service, increased by being supported by weak and failing abutments, without requiring any of the rivets to be re-riveted, and the annual reports of the bridge inspector do not mention the detection of any loose or defective rivets.

The writer thinks there is no doubt but that Mr. Clarke correctly conceived the reason that this type of truss has shown so good a record, namely, "diffusing the strains over a large number of points rather than concentrating them on a few." From long observation of the bridges on the New York Central Railroad, he has arrived at the opinion that solidly riveted lattice girders unquestionably give the best, safest and most durable service for such constant and heavy traffic as that required by the New York Central system, and especially so in cases where the masonry support is not of the best. He considers the riveted lattice girder, with a modern solid floor track system, as the ideal railway bridge of the present day, up to spans not exceeding 200 ft. Beyond that length of span, economy indicates the desirability of using pin-connected tension web members and lower chord bars.

Mr. Pegram. GEO. H. PEGRAM, M. Am. Soc. C. E.—It is the prime duty of the engineer to save money in maintenance and construction, but it is his highest obligation to insure safety, and to the conscientious engineer a proper balance of these conflicting motives causes a great deal of anxiety. In designing a bridge, the specification of unit stresses enables him to proportion the parts with exactness. It would now seem wise to fix the limiting stresses governing the life of the bridge, which should be applied with the same precision in deciding upon its removal, and it is only by a consensus of opinion that these limiting stresses can be fixed.

The writer's observations agree with those recorded in the paper in the extent to which material has been overstrained with apparent safety. He has known numerous cases where wood has been used continuously in stringers at a stress of over 2 000 lbs., and where iron has been used, particularly in Howe trusses, at a stress of over 20 000 lbs. He is inclined to agree in the limiting stresses which the paper suggests may be used, but these can hardly be expressed as a definite percentage more than the bridge was designed for, because the stresses used in designing, as specified by different engineers, are not uniform.

The paper naturally brings up the question: What stresses shall be used in designing to secure proper length of life in a bridge? Iron bridges have had unreasonably short lives in the past on account of the rapid increase in the live loads. In future designs, special consideration should be given to this fact. The heaviest engine weight on the Union Pacific system increased over 60% in the ten years from 1881 to 1891. It hardly seems possible, however, that the heavy engines and trains now specified can be increased 50% on account of the limitations of cross-section.

The question raised as to the effect of a moving load compared with a static load must remain undecided until more experiments have been made on structures in service to determine the actual stresses in the various members and parts of members, and the writer inclines to the opinion that such experiments will modify considerably existing ideas with respect to details as well as to the comparative effects of live and dead loads, for it must be admitted that some of the "flimsy constructions" have given better service than some of the more rigid and confidence-inspiring bulky constructions.

A large gusset plate covered with rivet heads has an appearance of strength not always realized. Similarly, the writer knows of several cases in which light intermediate posts and bars of trusses have been bent by being struck, and the bridge saved from destruction by their bending out of line, gradually arresting the load by their deflections, whereas a large end post has been broken short off.

While improved materials and methods of manufacture might justify an increase in the unit stresses used in designing, engineers would

probably not agree to much increase, and wisely, because the unit stresses used 20 years ago were founded on experiments, and the conclusions drawn have been confirmed by subsequent experience. The evolution in specifications has been largely in the line of requiring more labor to produce practically the same results. The writer thinks it will be generally admitted that safe limiting stresses for a dead load, assuming wrought iron as the material, are 15 000 lbs. tension, and 12 000 lbs. compression, reduced by the usual Rankine column formula. Mr. Pegram.

In this connection the writer would indorse using one formula for all compression members, regardless of end fittings, which practice is now becoming more usual, and would make this formula the Rankine-Bouscaren pin-end column formula. This formula is the most theoretical in form of any used; it was originally based on experiments and has been supported by all subsequent experiments, and the numerous published tables make its application as easy as any other.

The straight-line formula, which was introduced for simplicity of application, seems to have opened a field for originality in the variation of constants, which has been so assiduously cultivated that it is impossible to make tables which will be of general use, and some of the modifications lead to absurd results.

It is so much more difficult to produce accurate fittings with square bearings, that in most cases the members are really weaker than those with pin bearings. Particularly is this the case in top chords where the camber must be taken out of the bridge before the bearings are theoretically square. It must also be remembered that a top chord will not stand as much proportionately in a testing machine as an intermediate post, because the former contains plates slightly curved between the rivets in the process of manufacture, while the latter is composed of channels which do not have this initial distortion.

If it were possible to be certain of the live load stresses, the unit stresses could be made the same as those specified for the dead load, but with the doubt existing in respect to impact and past experience in the increase of the loading, a margin must be allowed, based largely upon opinion. It is certain that the mass through which the live load stress is distributed will lessen its injurious effects, especially those caused by unbalanced parts, low joints, etc., and as the mass is less in short spans than long spans, it would seem reasonable to use the old method of making the impact addition decrease with the length of span. The writer would suggest an addition to the live load for impact varying from 50% at 15 ft. length of span to 10% at 100 ft., this percentage being added to the stresses produced from the loading and the sum considered as equivalent to a stress from static load. This is substantially the best practice of 20 years ago with a reasonable addidition for increased speed. It would then seem wise to allow a margin

Mr. Pegram. of 50% for future increase in the live load, so that in designing a bridge the dead load stress would be taken as given above, and the live load with impact added at 10 000 lbs. tension and 8 000 lbs. compression reduced by the column formula. Where the total stresses in a bridge exceed those specified for dead load, the bridge should be replaced.

These limiting stresses may seem conservative, but it must be considered that it is necessary to keep within the elastic limit, and the distribution of stress through a member or among several members is not uniform; posts may bear on the corners only; of two diagonal ties, one may carry all the load or one may be injured. The writer recalls several cases where the margin of strength has enabled a bridge to carry traffic after accidental injury until repairs could be made conveniently, and the savings of expense in such cases are often very large; but the most cogent reason is that the specified limits are generally exceeded.

For the purpose of comparing the unit stresses used in different specifications with those herein proposed, the following table has been prepared, the stresses given being those obtained by dividing the total stress in a member from dead and live load considered as static, by the section obtained under the specifications. The specifications taken, which the writer happens to have at hand, are, those published in 1890 by G. Bouscaren, M. Am. Soc. C. E.; those published by Theodore Cooper, M. Am. Soc. C. E., in the same year; the 1887 specifications of C. C. Schneider, M. Am. Soc. C. E., and the author's specifications, dated 1894. Mr. Schneider's later specifications are for soft steel, in which higher stresses are apparently taken than were previously used in wrought iron, and are therefore not used in comparison.

	Bouscaren.	Cooper.	Schneider.	Greiner.	Proposed.
Tension. Chords—					
200-ft. span	10 000	9 600	10 110	10 000	11 250
500-ft. span	12 000	10 667	11 070	11 225	12 000
Compression. Chords—					
200-ft. span, 30 radii	7 500	8 521	8 900	8 280	8 580
200 " 45 "	7 500	7 980	8 310	7 750	8 100
500 " 30 "	9 000	9 467	9 740	9 450	9 156
500 " 45 "	9 000	8 867	9 100	8 920	8 640
Intermediate posts—					
200-ft. span, 65 radii	5 670	5 090	7 180	6 630	7 140
200 " 130 "	3 640	2 080	4 360	4 360	4 600
500 " 65 "	6 400	5 600	7 810	7 725	7 570
500 " 130 "	4 110	2 290	4 740	5 450	4 880
30-ft. Plate girder—					
Tension	7 116	8 000	8 013	7 963	7 550
Compression	6 508	*7 320	*7 333	7 050	*7 000
60-ft. Plate girder—					
Tension	7 360	8 000	8 570	8 355	8 550
Compression	6 560	*7 040	*7 500	7 400	*7 640

* Same gross section as tension flange.

Mr. Pegram.

The increase the author shows in the rolling load, and therefore in the stresses to which bridges have been subjected, has been very general. In the face of such facts, the microscopic refinement to which calculations are carried seems absurd, and the waste of time caused by elaborate specifications seems inexcusable. Particularly is this the case in the persistent use of the engine wheel diagram. It was shown in a discussion of the paper* by Joseph M. Wilson, M. Am. Soc. C. E., on "Specifications for Strength of Iron Bridges," that a uniform load in combination with a single concentrated load would give the stresses obtained by the engine loading with practical exactness.

It was further shown† in a discussion of the paper by Theodore Cooper, M. Am. Soc. C. E., on "American Railroad Bridges," that this equivalent loading could be made to include the effects of impact, thus abridging in a large measure the labor of calculations. It would seem as if the use of such equivalents, or the old method of using equivalent uniform loads, might be used with advantage. The actual engine loading must, of course, be the basis for any such equivalent, and might reasonably be stated in the specifications for the information of non-experts as the loading designed to be covered.

With respect to stresses in wood, consideration must be given to the fact that while wood does not lose strength, with age, to resist gradually applied loads, it loses fiber and ability to resist shocks. Some years ago the writer made some experiments on pine of different ages, and found that while the pieces gave practically equal resistances under gradually applied stresses in the testing machine, they were broken by the same weight falling from the following average heights: 13 ins. for new seasoned lumber; 10.6 ins. for lumber in service four years; 6.7 ins. for that in service nine years. Engineers are so accustomed to a large margin of strength in wooden structures, which are so often overstrained, that it hardly seems wise to make much increase in the unit stress. Although the writer believes the limits given in the paper might be safely used, he considers a unit stress of 1 200 lbs. per square inch for bending in yellow pine about right in designing new work, and a limit of 1 800 lbs. in removing old work.

Mr. Montfort.

R. Montfort, M. Am. Soc. C. E.—A bridge designed in accordance with the best modern practice would undoubtedly carry safely and without injury to itself, loads producing strains in its members 50% to 65% in excess of those for which it was originally proportioned. It has been the writer's custom to consider as safe a truss bridge of 125 ft. span or over, so far as the lower chords and main ties are concerned, if the strain per square inch in them does not exceed 15 000 lbs. Unfortunately, however, the details of old bridges are very rarely of equal strength with the bodies of the principal members, and, therefore, they

* See the *Transactions* of the American Society of Civil Engineers, Vol. XV, p. 474.

† See the *Transactions* of the American Society of Civil Engineers, Vol. XXI, p. 575.

Mr. Montfort. determine the life of the structure in a very large majority of cases. Thus, cast-iron joint boxes are found in many old bridges, while in a few still remaining, entire compression members made of the same material are found. The fact that a bridge has cast-iron joint boxes or members made entirely of cast-iron would not of necessity demand its condemnation, yet their existence, especially if poorly designed, might lead to its condemnation while the strains in the bodies of the principal members are well within the limit of safety. The writer has seen a cast-iron top chord removed from a 200-ft. span. which had been in service about 20 years and had carried loads more than 50% greater than that for which it was designed, show, when broken for the scrap pile, a thickness of metal on one side of less than $\frac{1}{8}$ in., while the opposite side was $1\frac{1}{2}$ ins. thick, due to the shifting of a short portion of the core used when making the casting and which was not discovered, although test holes were drilled close to the point where the displacement occurred.

Again, want of rigidity or of proper lateral bracing should be considered when passing upon the safety of a bridge. In most old structures the attachment of the laterals will be found defective and not sufficient to develop their full strength, while in others little or no provision is made to transfer the stresses from the lower lateral rods of through bridges into the bottom chords, or make other proper provision for the component of the stress in these rods now usually transferred into the chords.

The fact that until 20 years ago it was almost the universal custom to make eye-bars by welding the heads to the bars should not be overlooked. While the welds were generally made with great care, and the bars afterward tested to a strain of 20 000 lbs. per square inch, yet there have been numerous cases of failure under service in these welds. The writer has seen broken bars which showed only one-third of the section perfectly welded, yet they had withstood a strain of 14 000 lbs. per square inch under service for 15 years before breaking. He has never known a principal member of a bridge to fail in its body where a serious flaw did not exist. All of the failures of tension members that he has known of took place in bad welds or the minor parts, such as the smaller system truss rods, floor-beam suspenders and floor-beam hangers. He has seen truss rods with screw ends break in the screw when the figured strain on a section taken at the base of the thread was less than 12 000 lbs. per square inch, but this was probably due to the imperfect bearing of the nut. Floor-beam suspenders and hangers in old bridges fail probably more often than any or all of the other parts taken together, and frequently under extremely low strains. He has in mind three failures of hangers where the figured strain was only 7 000 lbs. per square inch, and all took place in similar spans and within a period of one year, the structures having been in service for 14 years before a break occurred. These hangers were of the usual

U shape, with screws and nuts, but there was only one hanger at each joint; the tendency of unequal strain, which is so likely to occur where two hangers are used, was, therefore, much reduced. The fracture occurred in all cases in the center of the hanger directly over the pin, where there was a full section and no weld. The hangers were made of 1¼-in. round iron and rested directly on rough circular castings, which rested in turn on the pins. The failures were probably due to the imperfect bearing between the rough round hangers and the rough castings on which they rested, combined with an inferior quality of iron. Mr. Montfort.

J. P. Snow, M. Am. Soc. C. E.—The consideration of unit strains alone is not sufficient to determine whether or not a particular bridge should be renewed. In fact the units used by bridge engineers in designing new work are not derived from the scientific consideration of laboratory tests on materials, but from the study of structures in service. How else have they become reduced so much from what obtained 20 years ago? What else has brought down the units for steel construction from the estimates used when it first entered largely into bridge construction? Let it be supposed that a man is endowed with all the technical education that the world can bestow, except a knowledge of practical bridge designing; he shall be acquainted with all the experiments and tests that have ever been made on iron and steel; he shall have at hand all the multitude of typical engine diagrams and classes X, Y and Z loadings that locomotive builders have created or bridge designers evolved; but shall never have seen a specification, a set of plans, a bridge, or have any knowledge of the unit strains usually employed. He shall be called on to design a bridge, and by some unexplainable chance shall decide on a truss and floor system like the best modern development; in fact he shall be fully equipped for a full-fledged designer except that he shall have had no experience or knowledge of prevalent practice. Does any one think that he would adopt a scheme of unit strains that would produce a satisfactory bridge? He would adopt strains based on the elastic limit probably, but they would be far above what is considered good practice, very likely about twice what is generally used. Mr. Snow.

What, then, is the basis of the present units? Examinations of structures in service unquestionably. Any one who has seen a light bridge writhe and sway under a heavy train will understand why unit strains must be kept low. Probably the great majority of engineers copied their units from recognized authorities or were brought up under engineers who so copied them; but without question these low units originated with some one who studied his bridges in service and gradually lowered the units until a satisfactory structure was produced. The experiments on fatigue and similar subjects have had an effect in modifying the units used in the various parts of a truss, but

Mr. Snow. are not the basis on which the fundamental unit rests, and is it not just as scientific to experiment with a bridge as a whole as with little strips of its individual parts? The experiment need not be carried to rupture in order to ascertain whether or not it is satisfactory.

In order that a bridge may be satisfactory it must remain rigid under trains and deflect but very little. These conditions can only be obtained by using low unit strains. The practice (more markedly prevalent some years ago than now) of using much higher units on steel than on iron has always seemed inexplicable to the writer. Certain units, like shearing of rivets, and bearing on rivets, pins and rollers, may well be high in steel; but putting tension and compression (the strains that give rise to deflection) high for steel and low for iron when providing for a live load and calling the resulting design satisfactory seem illogical. The theorist may say it is right because the units compare with the elastic limits, but the practical man says it is not right because a light steel bridge is no stiffer than a light iron one; in other words, the units do not compare with the moduli of elasticity. Bridges are not built to resist breaking alone. It takes a very poor bridge to fall. They will become unsatisfactory and give rise to wholesome fear a long time before they will fail utterly. Units should be selected largely on the basis of resisting deflection. A girder having a generous ratio of depth to span may well be designed with higher units than a very shallow one.

The life of a bridge depends in large measure upon its rigidity. The calculated unit strains assist greatly in directing attention to weak parts and connections, but the limit between reasonable safety and danger cannot be fixed absolutely by the strain sheet. Study of the action of a structure under strains must be combined with a study of its strain sheet in order to reach a logical and defensible conclusion. Cases II and IV cited by the author show this; for, whereas an over-strain of 141% in the plate girder did not condemn it, an over-strain of 144% in the Bollman truss did. The 3% excess could hardly have been the determining factor. Cases are also mentioned in the paper of main rods in Bollman trusses snapping short off, showing that they had been strained beyond their elastic limit many times. Is it not the lack of rigidity in this class of bridges that allows oscillation which produces strains beyond the computed amount? Vibration, or "structural motion" as George H. Thomson, M. Am. Soc. C. E., calls it, is surely an enemy to the life of a bridge.

It has been the experience of the writer that loosely built bridges, like the early pin structures, become unsatisfactory and call loudly for renewal, while others of more rigid type stand satisfactorily, although the strain sheets of the latter class may show the larger unit strains. On the Boston and Maine Railroad the same tension unit is used in new work on built plate girders as on eye-bars, and 10% more on rolled

beams. Although this is not in accordance with accepted practice in certain high places, still, in the opinion of the writer, the beams and girders then make better and more serviceable bridges than do the trusses. Mr. Snow.

The units suggested by the author as the highest permissible for iron bridges seem to be on the safe side. Certainly in the case of rolled beams and plate girders it is likely that higher strains would not cause trouble if the track is kept in perfect condition. However, it does not pay to carry a bridge too long. If it is known that it must come off in a few years at best, the saving of interest on its cost is not to be considered beside the feeling of security that a new structure gives.

Timber stringers of long leaf pine are reasonably satisfactory with below 1 800 lbs. fiber strain. That is the limit at which such bridges are classed as unsatisfactory on the Boston and Maine Railroad, but they are by no means condemned. When strained to 2 200 lbs. they will carry the load, but will deflect disagreeably; 1 200 lbs. is the unit used in designing. For some reason both timber and iron will bear in a satisfactory manner a higher fiber strain when acting transversely as a beam than when acting longitudinally as a strut or tie.

The remarks of the author regarding poor track are very pertinent. A light bridge will last much longer with a perfect track upon it than if the rail is light, joints poor and alignment bad. This is another reason why the strain sheet must not be the only guide in condemning bridges. If a structure has been shaken 40 times a day for a series of years on account of a poor track, the figures will not show it, but the bridge itself will. Occasionally it becomes the disagreeable duty of the bridge engineer to condemn a bridge when the rest of the world think it good enough, but it has been the experience of the writer that generally the complaint comes from the operating or maintenance departments that a bridge is unsatisfactory, and it behooves the engineer to show cause why it is all right.

While the writer agrees with the safe limits of strains fixed in the paper, yet he believes there are few cases in which they will be the determining factor in deciding whether or not a bridge is safe.

THEOD. G. HOECH, M. Am. Soc. C. E.—In 1872 Dr. Strousberg, a well-known German railway organizer, erected a bridge of six spans near Forst, Germany, the wrought-iron superstructure coming from his own shops. After the railway was purchased by the Prussian government a thorough examination revealed numerous cracks as deep as 3 ins. in the chord plates of the pony trusses of the bridge. Some of these had been obviously patched up during construction. The bridge was replaced in 1893 with a new one of similar design, and the worst part of the old superstructure was carefully removed to properly arranged supports and tested to destruction.* The result was surprising. Mr. Hoech.

*See the *Zeitschrift für Bauwesen*, 1895, pp. 289–316.

Mr. Hoech. Under a uniform load of 6 759 lbs. per linear foot of track, the upper chords in one of the middle panels bent aside, the skew of 58° increasing the bending strains after deformation, and the pony trusses of the 99-ft. span settled upon the timbers piled 1 ft. underneath. The bridge withstood a stress of 38 000 lbs. per square inch in the chords, and failed because the horizontal lattice with tie plates between the halves of the chord members did not develop the full strength of the H section. The breaking test proved that a well-designed Schwedler bridge would have withstood much greater strains than those caused by the traffic, despite the old cracks, crystalline spots and slag flaws in the wrought iron. The surface of the metal was free from rust, although only ordinary care had been exercised in maintenance; no rivets had been broken, and the cross-beams and stringers, with their connections, were not damaged. It is worth noting that the vertical bending of the loaded trusses, when in service, did not indicate the fatal weakness of the upper chords.

Mr. Schneider. C. C. SCHNEIDER, M. Am. Soc. C. E.—The writer thinks good judgment was shown in the paper in making such a radical distinction in the permissible strains allowed for new bridges, where provision should be made for future contingencies, and the strains which are considered the limit of safety for bridges in actual service.

However, he does not agree with some of the author's statements and conclusions. For instance, he does not agree "that it is positively certain that the method of figuring stresses from concentrated wheel loads causes strains so far above what really exist as to almost class this method as unscientific." The author states that there can be no concentrated loads when there are stiff rails, etc. While it is true that there are no real concentrated loads such as are assumed in calculations (the loads on a bridge being distributed over two or three ties), this small distribution, however, will only materially change the effects of concentrated loads in exceptionally small beam or plate girder deck spans. On a 5-ft. span the bending moment may be reduced 25% to 30%, while in a 20-ft. span this reduction will be only about 5 per cent. On deck plate girders of larger spans, this distribution has very little influence, as the centers of application of the loads, as well as the end reactions, remain the same. This distribution, caused by the rails, would therefore only round off the angles of the polygon representing the moments produced by concentrated loads. In plate girder bridges with floor beams and stringers, as well as in truss bridges, the static effect of the loads will not be changed by this distribution, as the loads are distributed by the stringers over the length of one panel and concentrated at the panel points. It is therefore evident that the strains figured from concentrated wheel loads are not so very much above what really exists as to class this method as unscientific.

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The author seems to take it for granted that the prevailing opinion among bridge engineers is that a live load produces twice the strain that is caused by a dead load. The writer, however, believes that there are not many who hold that opinion. While it is generally admitted that a suddenly applied load produces about twice the strain of the same load at rest, an engine and train moving over a bridge is by no means a suddenly applied load. Assuming the average speed of a train to be 60 ft. per second, it would take two seconds to fully cover a bridge of only 120-ft. span with a live load. This certainly cannot be considered a suddenly applied load. The fact is, that the load of a moving train is applied gradually, one pair of wheels at a time. The theory of the suddenly applied load would therefore apply approximately to very small spans only, which receive their maximum load from one pair of wheels, which would generally be spans of 5 ft. or less.

The paper further states that "no one can deny that a stress less than the elastic limit for dead loads, and less than one-half the elastic limit for live loads, must be used in considering whether or not a structure is safe." The writer begs to deny that. The static effect of a live load *per se* is the same as that of a dead load, depending upon the amounts and distribution of the loads only. If a train were moving over a bridge at a very slow speed, the strains in the various parts of the structure would be approximately the same as the static strains calculated from the same loads, with the exception of framed structures, wherein the members, on account of the deformation of the system, receive secondary strains in addition to the direct strains, which, on account of their complexity and the labor involved in calculating them, are generally not considered, due allowance being made either in the usual margin allowed for safety or by adding a percentage to the calculated strains for that purpose. The dynamic effect of a live load, commonly called impact, depends upon the conditions under which the live load is applied. The conditions which affect the impact are the conditions of the track, the dynamic action produced by the deflection of the bridge, the action of insufficiently balanced drivers, the reciprocating motion and vibration of the machinery, and the velocity of the train.

As the static and dynamic effects of a live load depend each upon such entirely different conditions, it seems more rational to consider the effect of each separately and distinctly, in order to arrive at a more scientific solution of the problem of determining the safe working strains in railroad bridges. As the internal stress in a member of a structure is proportioned to its elongation or reduction, it is evident that it makes no difference, as far as the resistance of the material is concerned, whether this stress is produced by the weight of the structure, by the static effect of a superimposed load, or by the dynamic

Mr. Schneider. effect of a moving load. If a permissible stress per square inch is safe for one strain, it is safe for all strains. If, therefore, the dynamic effect of the live load could be determined, there is no good reason why a uniform permissible strain should not be used for the static as well as the dynamic effects of the loads.

The author admits the dynamic effect of a live load; he also admits that the shocks or blows are more severely felt in the members which receive these blows suddenly and directly and which have little inertia of their own to resist and absorb them. In other words, he admits that the dynamic effect varies with the length of the span. He also recognizes this fact in his specifications by allowing a certain permissible working strain (15 000 lbs. per square inch for static strains), and adding 100% for impact for all-live load and smaller percentages as the dead load, or, in other words, as the length of the span increases; but prefers to disguise the fact by a modification of Launhardt's formula, which has no scientific basis, nor is it derived from data taken from actual experiments or verified by practical experience. However, the author claims that this formula is based on the foundation of fact; so the writer requests him to state the facts upon which this formula is based.

As the paper condemns all other methods of proportioning bridges, the writer wishes to point out some apparent inconsistencies in the Baltimore and Ohio Railroad specifications which need explanation. If Launhardt's formula, which is based on the theory of the fatigue of metal, is correct for permissible strains in tension or compression, the writer can see no good reason why it should not be used for shearing and bearing strains, as well as for combined strains. If the dead and live load strains on a member are equal, the paper allows a working strain 50% greater in tension or compression than if the strains were produced by live load only; but for the strains on the rivets in the same member it makes no such distinction, but specifies a uniform strain. If splices in two tension members of exactly the same sectional area were proportioned in accordance with the specifications, the one receiving strains from an all-live load, while in the other the dead and live load strains are equal, there will result two members of the same section, each one with a different number of rivets in the splice. The member in which the dead and live load strains are equal will have 50% more rivets in the splice than the one which receives live load strains only, yet in both members an equal amount of metal is to be spliced.

For members which receive combined transverse and direct strains, the Baltimore and Ohio Railroad specifications prescribe a uniform working strain per square inch, irrespective of the length of the span or the proportion of the minimum and maximum strains. If Launhardt's formula is founded on facts, then a uniform working strain is wrong.

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There is either not enough material in these members if they belong to short spans, or a useless waste of material if they are members of very long spans, if proportioned in accordance with these specifications.

The writer does not agree with the author that experience with structures which have been working for some years under an increased traffic without injury furnishes the only means of solving the question of safe working strains for railroad bridges. No doubt these experiences are very valuable and instructive, but there is a more direct and scientific way of solving this question, and that is to determine the strains produced by the live load on the various members of a structure by actual measurement. The late Prof. Fränkel, of the Polytechnic School at Dresden, made some very valuable and interesting experiments on structures in actual service, with an instrument designed by him for the purpose of recording the actual strains in members of bridges produced by a moving train under different velocities. The writer has for many years persistently advocated a series of similar experiments to be made on American types of bridges, but has so far received little encouragement. He would be glad to cooperate with any railroad company that would furnish facilities for making these experiments. Large amounts of money have been wasted on useless experiments, such as the tests made on full-sized compression members, which have not even furnished data to develop a rational formula for calculating approximately their strength. As, at the present time, the knowledge of the dynamic effects of moving loads on railway bridges is limited, the subject seems important enough to receive some attention. The results received from carefully and conscientiously made experiments in that direction would be of great practical value.

As the paper denounces the method of considering the dynamic effect of the live load as guesswork, the writer feels compelled to say a few words in reference to the subject and to give his reasons for adopting this method. In 1877, the late Prof. Winkler, of Berlin, in a paper published in the *Journal* of the Austrian Society of Architects and Engineers, called attention to the unscientific method used at that time in proportioning bridges, and suggested that the dynamic effect of the live load should be considered in addition to its static effect as a more rational way than that of considering the static effect only and allowing a large so-called factor of safety to cover ignorance. He also proposed a formula for determining this dynamic effect, which, owing to the absence of any data at that time, was based more on scientific assumption than on practical experience. Later on, this subject was discussed by various other authorities, all of whom seem to agree on the correctness of the principle of considering the dynamic action of the live load in proportioning the members of

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railroad bridges. Some of the older specifications also called for a percentage to be added for impact to the stringers, floor beams and hangers. The writer believes, however, that he was the first to adopt this principle systematically, using a uniform permissible working strain in the "General Specifications for Railroad Bridges," published by the Pencoyd Iron Works in 1887. Attention was first called to the subject by Prof. Winkler's paper, and owing to his high standing in the profession and the regard for his ability held by the writer, the latter was prompted to give this matter serious consideration. He was convinced by the soundness of Prof. Winkler's arguments that this was the only correct method to use in proportioning railroad bridges. He then commenced to collect all the available information on the subject. Some very valuable data were obtained from Prof. Robinson's report on the vibration of bridges. At that time he learned that Prof. Fränkel had been making for several years experiments on railroad bridges for the purpose of determining the actual strains produced by the moving load under different velocities. In response to his request, Prof. Fränkel, whose experiments were commenced, the writer believes, in 1881, and continued until his death in 1894, kindly supplied him with all the data collected up to the time and furnished from time to time the results of subsequent investigations. They were made on a number of bridges of different spans of the European types. While the number and variety of these experiments were not sufficient to furnish data for making very close calculations of the effect of impact, they were sufficient to establish some facts which had heretofore only been assumed; they also furnished enough information to serve as a guide to determine in a general way the probable dynamic effect of a moving train which would not be exceeded in practice. These experiments have developed the following facts:

First.—That the dynamic effect of a moving train varies with the velocity of the train and the length of the span.

Second.—That members which receive the live load direct are most affected, while those that receive the strains from the live load indirectly are less affected.

Third.—The dynamic effect diminishes as the parts are removed from the members to which the loads are applied.

Thus, the dynamic effect of the live load would be most severely felt by the floor stringers, less by the floor beams and floor beam hangers; next would come the web system, and the least affected would be the chords. In a deck bridge, however, with the ties resting on the top chord, the top chord would be the part most affected by the dynamic action of the train. From the data on hand the writer deduced coefficients of impact, which he believed would be in excess of what really existed under the most unfavorable conditions, adding 20% for secondary strains, which percentage should not be exceeded

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in any well-designed structure. Beam bridges and plate girders receive practically no secondary strains, but as very great refinements were not proposed, no distinction was made for plate girders. The writer hesitated a long time before using these coefficients for impact in actual practice; and not until he had applied this method to a great number and variety of bridges, to ascertain whether it would apply equally well to all kinds of structures and conditions, did he venture to embody this method in his "General Specifications for Railroad Bridges" and consent to have it published. These specifications have now stood the test of eight years, and have been used on ordinary trusses, trestle towers, arches, cantilever bridges, continuous spans and draw bridges and have in every case given satisfactory results.

If a new bridge is rationally designed and low working strains assumed in accordance with the present practice in order to meet future contingencies, it should be able to stand a small increase of the live load in all its parts. In a pin-connected span with vertical posts and diagonal bars, counters are required in those panels where the bars receive strains in an opposite direction. If the bridge is proportioned in accordance with specifications based on Launhardt's formula, no counter is required in a panel where the live load compression is equal to the tension produced by the dead load. It is evident that if the live load is increased that the live load compression in that panel would increase in proportion and therefore require a counter; that is, while the chords and most other members of the truss would be strong enough to resist an increased live load, there would be some parts which would have to be reinforced or patched up in order to resist an increased load. If, however, this bridge is proportioned in accordance with the impact method, the live load strain is increased by a certain percentage; and consequently in this panel, where the static strains from the dead and live loads are equal, owing to the addition of the impact to the live load, a counter would be required, thus making provision for a reasonable excess of live load above that for which the bridge has been designed. In a bridge designed in accordance with specifications using the impact system, the strength of the details and connections will be in proportion to the main members, as the impact applies to all parts. Another advantage of the impact method consists in its simplicity; using a uniform working strain throughout, the permissible strains for compression members, shearing and bearing values for rivets, etc., can be tabulated and the work of proportioning the members of a structure thereby reduced to a minimum. If a system is correct it ought to be applicable to all cases and all kinds of structures. The impact method fulfils this condition. The writer is using his specifications for railroad bridges for highway bridges and buildings, by simply changing the coefficient of impact. For highway bridges he adds 25% to the live load strains, and for buildings discards

Mr. Schneider. the impact altogether. If future experiments should prove that the coefficients for impact adopted are too large, they can be easily corrected, which, however, would not affect the system, but would only involve a change of the numerical values of the coefficients.

Considering the cases cited in the paper, with a view of learning some practical lessons from them, the writer fails to see that these facts upset any of the well established theories of mechanics and statics, as the author claims. As the structures under these excessive loads did not show any indications of failure, it is evident that the maximum strains produced by the combined static and dynamic effects of the loads have been within the elastic limit of the material. The author has calculated the strains per square inch produced by the dead load and the static effect of the live load, which would be enough below the elastic limit to be considered safe if the live load did not also produce a dynamic effect, which was not considered. Had the actual strains been ascertained by measuring the elongation of the various members during the passage of one of the heaviest engines, the results would have shown at once the maximum strains the material was subjected to in every one of these cases, and would have enabled the engineer to judge whether these strains were enough below the probable elastic limit to consider the structure safe for the present conditions of the traffic. It would also have given the dynamic effect of the live load, which could be applied to the calculation of similar cases. If it is supposed that the actual strains in these cases had reached the usual elastic limit of 26 000 lbs. for wrought iron, there would be the following dynamic effects of the live load for the first three cases :

Case I, 26-ft. span	65	per cent.
Case II, 20-ft. "	60	"
Case III, 84-ft. "	62.8	"

The dynamic effects of the live load, as given in the Pencoyd specifications for the above cases, are 72, 74 and 79% respectively. The remaining two cases the writer has not been able to analyze, on account of insufficient data. These cases prove the statement he made before, that the coefficients for impact proposed by him are in excess of those that really occur in ordinary traffic.

For the purpose of determining the limit of safety in existing structures, the writer would suggest using the impact system as given in the Pencoyd specifications, except for plate girders, for which he would make a reduction of 20% in the coefficient for impact, as they receive practically no secondary strains, and allow a working strain of 22 000 lbs. per square inch, properly reduced for compression; and calculate the fiber strains in the flanges by the moment of resistance of the cross-section. If a working strain of 22 000 lbs. per square inch is allowed as the safe limit for existing structures, the bridges designed in accord-

ance with the Pencoyd specifications would be able to stand the following increase of live load: Mr. Schneider.

25-ft.	span,	plate girders,	49	per cent.	
50-ft.	"	"	51	"	
75-ft.	"	"	53	"	
100-ft.	"	"	55	"	
150-ft.	span,		59	per cent.	
200-ft.	"		62	"	
300-ft.	"		70	"	
500-ft.	"		80	"	

J. E. GREINER, M. Am. Soc. C. E.—In the body of the paper the author endeavored to lay force upon certain points, and, in closing this discussion, these points will be considered separately and in the order of their occurrence in the paper. Mr. Greiner.

I. "Owing to an increase in train loads and flimsy construction the life of railroad bridges heretofore has been scarcely 25 years, but the life of a bridge of good design will be determined by the care bestowed upon its maintenance rather than by tonnage or years, provided specified loads are not exceeded. If placed in the hands of indifferent and incompetent persons, there will be a greater certainty of its failure before than of its existence after 25 years."

Mr. Stanwood has presented some interesting illustrations showing the actual loss of section caused by corrosion due to neglectful maintenance, and their value as records of the actual amount of deterioration per year cannot be overestimated. Structures subjected to the same evil influences are met with on almost every railroad, and if they stand for a period of 25 years, the fact can be attributed mainly to the intelligence bestowed on their maintenance. There are effective means of protecting iron against such injury, and if some one of these means is not employed, the same results as pointed out will take place sooner or later.

Attention has been called to the satisfaction given by multiple system riveted trusses on the New York Central Railroad. Some of these bridges have for years carried a traffic much greater than was contemplated at the time they were built, and their great stiffness and durability are attributed to the fact that strains are diffused over a large number of points instead of being concentrated on a few. Mr. Snow states that it has been his experience that loosely built bridges, like the early pin-connected structures, become unsatisfactory, while those of a rigid type are doing their work all right, although strain sheets indicate higher unit stresses. This is a very general experience and has led to quite an extended practice of designing bridges so that they will have a small number of large-size members rather than a larger number of smaller members, thereby concentrating strains and metal instead of diffusing them. The author ventures the opinion, that had the bridges on the New York Central been of this character, they would have stood just as well as the bridges mentioned, and that their stiff members and firm connections may have contributed more to their rigidity than the fact that the metal and strains were diffused.

Mr. Greiner. II. "The avowed purpose, when designing a bridge, is to develop a structure which will safely carry the specified loads. If this structure will be perfectly safe under a greatly increased load, then, from an engineering sense, more material has been used than was necessary for the avowed purpose, and this waste cannot be considered good engineering."

Mr. Le Conte, while admitting that engineers are placing more material in their large bridges than is necessary for carrying the specified loads, will not acknowledge that any excess has been used in small spans. The author understands that Mr. Le Conte would approve of somewhat higher unit stresses than are commonly used for large spans, but would not endorse an increased stress in small spans. Considering the fact that bridges are designed so as to carry a future increase in traffic of a very indefinite extent, the author would not use higher stresses than specified. The waste in material is not because the bridges are built too strong for future emergencies, but because they are being built too heavy for their avowed purpose of carrying in safety a certain specified load. The low stresses usually employed have not been so commonly adopted, merely because experience has demonstrated them to be safe, but rather because experience has demonstrated that when a bridge is nominally designed to carry an 80-ton engine and tender it will very likely be called upon to carry 120-ton engines. Railroad companies are led to believe that they are getting structures proportioned for a definitely specified load, but they are paying for structures really designed for much heavier loads; in other words, stress sheets represent more than they profess. Would it not, therefore, be much better engineering practice to design bridges strictly for the maximum loads specified and take these specified loads from 50% to 100% in excess of immediate requirements? If this were done, unit stresses could be increased to the limits which experience justifies, and when the train loads have exceeded those for which the bridge was designed, it will have reached the end of its usefulness. Stress sheets would then represent all they profess, but not more, and railroad companies would be paying for what they expected to buy, but not for more. It is needless to state that such an innovation will not take place in the near future, owing to various prejudices, but it will be safe to predict that when the increase in locomotive and train loads ceases, as it surely will, engineers will then design strictly for the heaviest possible loads, or else they will be subjected to the alternative imputation of either specifying loads less than are in service, or of being extravagantly wasteful in the use of material.

III. "The condemnation of a bridge is a case for figures, experience and judgment, and involves a responsibility which should not be left to the unsupported opinion of the practical bridge man, or to the impetuosity of the theoretical alarmist."

Mr. Morison refers to a case where the calculated bending stress in the extreme fibers of bridge pins was found to be from 175 000 to

250 000 lbs., but when the pins were removed they were found perfectly straight. The stresses never existed. If such cases of calculated overstrain should be referred to many purely theoretical men, they would in all probability have ordered the bridge to be trestled at once, thereby involving a large amount of useless expense. Mr. Greiner.

Mr. Hazlehurst has known bridges to carry a heavy traffic for many years with entire safety with a unit stress that would horrify a man not familiar with, or one who was not accustomed to watch, the behavior of structures under heavy loads.

Mr. Yeatman calls attention to a bridge which carried for years, without ill effects, loads producing a calculated pressure on bearing area of rivets in web of floor beams of from 36 000 to 40 000 lbs. per square inch. The author knows of several plate girders, the rivets in which were strained equally as high without sign of weakness, and has met with two cases of box-beam girders, which failed by the rivets in the bottom flange shearing off bodily for a distance about equal to the depth of the beam, although the calculated stress was but about 60% or 75% more than allowed by common practice. These bridges had been examined by the author not more than three months previous to their failure, and to his personal knowledge there was nothing to indicate that the rivets would shear off. The bridges had also been inspected each month by the regular bridge inspector, and reports showed them in good condition up to the time of their failure. These bridges, therefore, while overstrained, gave no evidence of weakness before actual failure, and were no cause of uneasiness to maintenance officials. Mr. Snow says it takes a very poor bridge to fall. These were possibly poor affairs, although they fulfilled their mission, but they did not "become unsatisfactory or give rise to a wholesome fear a long time before they utterly failed."

Mr. Hoech calls attention to a bridge in Germany which, after 21 years' service, was found to have numerous cracks as deep as 3 ins. in the chord plates of the pony trusses of the bridge. The bridge was condemned, and when tested to destruction it withstood a stress of 38 000 lbs. in the top chords, despite the defects which caused its condemnation.

The failures of rods and hangers mentioned by Mr. Montfort are fully as instructive as any of the above, and the three failures of U hangers at 7 000 lbs., which broke directly over the pin, were similar to those mentioned by the author as having broken at 12 930 lbs. at the side of the pin instead of through the threads, where the section was larger. The author has met with many cases of broken hangers, but never any which broke at a less stress than the case mentioned in his paper, and the evidence given by Mr. Montfort will cause him to fix upon a lower limit than given. His experience with eye-bars also accords with that of Mr. Montfort; they usually break through

Mr. Greiner. the welds. Eye-bars which have been in service for over 20 years, and for a long period at a stress of 14 000 lbs., have stood without failure, although they were found to have cracks as much as ¾ in. deep on the outside of the head. When broken the heads are usually one mass of crystals. There is a case of failure of a Howe truss which may be interesting in this connection. The bridge was 140 ft. long, and was correctly designed for an 86-ton engine and tender, followed by a train load of 3 000 lbs. per foot, and was built during the end of the year 1888. In the meantime train loads had increased 25%, and, as there was much shifting over the bridge and it began to shake under this shifting, it was trestled at the center in the fall of 1893. The trestle bent was put in place by practical bridge men, without engineering supervision, and, in order that it would run with the stream, it was placed so that the cap would meet the chords at an acute instead of at a right angle. It supported the outer stick at a distance of 4 ft. and the inner stick at a distance of about 2 ft. from the center panel point. The weight of the dead and live loads was, therefore, transferred to the bent by means of a bending in the lower chord. The lower chord was composed of four sticks, 7¼ x 14 ins. During July, 1895, the lower chord pulled apart 2¼ ins., and an examination disclosed the fact that three sticks had practically broken in two, not at the splices, as would be supposed, but through the body of the sticks in the neighborhood of the panel point near the center of the bridge. The fourth stick was not broken, but had pulled a little at the splices. In addition to the direct breaks, there were cracks about some large knots in the outer stick, which indicated that it had been broken by transverse strains. In addition to the above breaks, the end bottom chord broke in the center of the first panel, which was due partially to the transverse strains caused by the floor system, which rested on the bottom chords, and partially to the fact that the timber bed-plate had been placed outside the line of action of the inclined end post. No accident resulted from these breaks. There was nothing in the appearance of the surface of the timber, which was white pine, to indicate that it was not in fairly good condition, and experience with the same kind of timber shows it usually lasts eight years or more before it becomes seriously unsound, and it was reported fair up to the time of the failure. An examination at the points of failure showed that the sections of the chords were either what would be called lifeless timber or were positively decayed.

Experience with this bridge should teach three good lessons:

First.—In placing a trestle or bolster under a Howe truss, or any other truss, it should be placed directly under the panel point, not on one side of the panel.

Second.—The fact that timber is only about seven years old should not cause an undue confidence in its condition. In this case the lower

chords were just as badly decayed in less than seven years as some other timbers are after twelve years. Mr. Greiner.

Third.—Bridges designed for regular traffic are not always fit for shifting purposes.

IV. "The terms 'safe working stress' and 'maximum stress' have no definite meaning, owing to the various ways engineers have of providing for the dynamic effects of live loads. Static stress can be figured, impacts must be guessed. When the two are combined to form what is called the total stress, the result is a guess and the actual figured stresses are obliterated. It would therefore appear better practice if engineers would preserve the stresses which they can figure and provide for dynamic effects by varying the unit stresses in the different spans and members inversely as the effects of shock vary."

In order to provide for the dynamic effects of moving trains, defects in material, for bulk in members, and to be a little safer than just safe, the author stated that "no one can deny that a stress less than the elastic limit for dead loads and less than one-half the elastic limit for live loads must be used in considering whether or not a structure is safe." Mr. Schneider says he can deny it. As stresses must be either less, equal to, or more than, a stated amount, and it is denied that they are less, it follows that they are equal to it or more. In order to sustain his denial it should have been shown that dead load stresses can be taken equal to or above the elastic limit, and live load stresses equal to or more than one-half the elastic limit. As this would be quite contrary to Mr. Schneider's practice, the author cannot understand the force of his positive denial, unless he means that by increasing the figured stresses by the addition of impact percentages he can use one common unit stress for both dead and live loads. He does this in his specifications, and while he uses a uniform unit stress approximately one-half the elastic limit, when this stress is separated into its two component parts, the one for the dead load and the other for the live load considered without additions for the impacts, there will result stresses which are below the elastic limit for dead loads and below one-half the elastic limits for live loads, a statement which has been denied. This will hold true for his limiting stresses also.

Mr. Schneider says that the formula used by the author disguises the fact that 15 000 lbs. per square inch is employed for static stresses, and 100% is added to the figured stresses for the dynamic effects of an all-live load. Is not the reverse equally true, or in other words, does not the 100% impact and a single stress of 15 000 lbs. simply disguise the formula $7\,500\left(1+\frac{\text{min.}}{\text{max.}}\right)$? It therefore appears that there is no practical difference in the results of Mr. Schneider's method and that used by the author. The one disguises the statement of the other, that is all; and since one represents the fact that stresses less than the elastic limit for all-dead, and less

Mr. Greiner. than one-half the elastic limit for all-live, loads are used, the other recognizes precisely the same statement, and the denial of the author's assertion has not been sustained.

The author does not condemn all methods of proportioning bridges other than the one proposed by Joseph M. Wilson, M. Am. Soc. C. E., in his paper on "Specifications for Strength of Iron Bridges."* As was stated in the paper the different methods make no material difference so far as weight and strength of bridges are concerned when designed for the same loads. It is the lack of uniformity in unit stresses as used by different engineers which were condemned as confusing to those who have not the time or inclination to look into the allowances which have been made for impacts. In order to compare unit stresses, they must all be reduced to the same basis, and it surely would be more consistent practice if this result would take place. Few will deny that engineers have much to learn concerning the dynamic effects of moving trains, and while the experiments quoted by Mr. Schneider indicated certain results in a particular case and gave a basis upon which to make guesses as to the probable effects in other cases, can it be asserted that the percentages, no matter how intelligently worked out, are anything more or less than guesses ?

The author has been asked to mention the facts upon which the formulas used in his specifications are based. These facts are mentioned in the paper on page 299, and are substantially the same as those upon which most specifications are founded, the difference being that in the impact percentages to figured stresses, the sum is considered as the total stress, while it may or may not be the total stress, and in the proposed method the figured stresses are surely what they represent to be, namely, stresses produced by the loads considered as static.

The various methods of arriving at unit stresses may be mentioned in this connection as indicating the different means of obtaining the same ultimate results.

First.—Impacts for counters, hangers, floor beams, stringers and spans under 100 ft., and a unit stress the same for live and dead loads.

Second.—Impacts as above, and unit stresses for dead load 50% greater than for live load.

Third.—Impacts for all spans and all members, and a unit stress the same for live and dead loads.

Fourth.—No impacts and dead load stresses 100% greater than those for live loads.

Fifth.—No impacts and variable unit stresses.

All of the above methods are in use and each has its advocates. The author therefore should not be condemned because he has selected the method which appears to him as the most rational, and has inad-

* See the *Transactions* of the American Society of Civil Engineers, Vol. XV, p. 389.

vertently criticised other methods with the hope that more specifi- Mr. Greiner.
cations will in future follow those prepared by Mr. Wilson, so that when an engineer speaks of a strain or stress in a bridge it will be known what he means, and when he speaks of a safe working stress it will be generally accepted as meaning that no impact additions have been made to the figured stresses.

As Mr. Schneider has pointed out what he considers as inconsistencies in the Baltimore and Ohio Railroad specifications and has asked for an explanation, it behooves the author to make reply.

To be consistent, the Launhardt formula should be applied to shearing and bearing values of rivets, etc., but consistency is not a virtue in this respect. These values are taken low enough to meet the worst effects of a moving load. They relate to details, and when details are strong enough to develop the strength of the members which they connect, a little more strength in some cases than just sufficient will not impair the safety of the structure, will add practically nothing to the cost of the bridge, and will avoid a refinement in calculations where such refinement will not justify the labor involved. In the case of the two members having equal section, the one being subjected to an all-live load and the other to equal live and dead loads, the two members would, as Mr. Schneider states, have a different number of rivets in their splices. Although the sectional area of the members may be the same, the figured stresses are different, and the rivets would be proportioned for the stresses, not for the area of section. There would be a sufficient number for the case of all-live load and more than sufficient for the live and dead load. Can any one seriously object to this when the excess pointed out happens only in extreme cases and involves so little material?

The Launhardt formula is not applied to combined stresses in chords of bridges, because the author considers bridges having crossties resting directly on the chords unsatisfactory, except in riveted trusses of spans between the limits of 80 ft. and 100 ft., and owing to the lack of accuracy in determining the stresses, which are affected by deflection, splices and position of pins, he would prefer not to use this arrangement in larger spans. Between the narrow limits of 80 ft. and 100 ft. the unit stresses would vary to so slight an extent as not to warrant any change, and a fixed value in accordance with good practice was therefore assigned.

The comparison of the two methods, when applied to counters in truss bridges, is true enough from a theoretical standpoint, and the specifications should have stated that web members not subjected to counter strains by the specified load must be designed to withstand the counter strains caused by an increased load of 100%, or else that counters must be provided for one panel in addition to the requirements of the specified load. Practically no engineer in good standing

Mr. Greiner. would leave out counters in the panel where the live-load compression is equal to the dead-load tension, or where these opposite strains are so nearly the same as to cause doubt.

In comparing the author's specifications with his own, Mr. Schneider should remember that the former specifications were made to meet the conditions existing on the Baltimore and Ohio Railroad. They are special, not general, and have a limited application. On the contrary, Mr. Schneider's specifications are general, and are designed to meet the requirements of any case whatever, and therefore their consistency is with them a virtue. The Launhardt formula can also be consistently applied, should it be desired, and the author still believes it would tend toward more uniform practice should engineers vary the unit stresses instead of varying the figured stresses by percentage additions.

V. "The unit stresses of 750 lbs. for white pine, and 1 000 lbs. for Georgia yellow pine, as given in text books since Lanza's experiments, are absurdly low, because good and safe practice sanctions much higher stresses."

When the author mentioned absurdity in connection with working stresses applied to rotten timber, he did not mean that in fixing the sections of timber stringers it is absurd to consider and provide for future possibility of decay, as Mr. Bates has understood it. The absurdity does not refer to making a reasonable provision for future deterioration, but in advocating a fiber stress as low as 750 lbs., when good and safe practice has justified a much larger stress. Of course timber cannot always be renewed or strengthened on the first appearance of decay, but when decay has attained such an extent that the amount of sound timber becomes an uncertainty, it is time to renew it and it usually is renewed, no matter whether the figured stresses on the nominal section be 1 500 or 750 lbs. The primary defects in timber, such as knots, shakes and sap, as well as season cracks and decay, must surely be considered in proportioning timber beams as well as in fixing upon what limiting stresses should be permitted, and when advancing the statement that 1 600 lbs. for white pine and 1 800 lbs. for Georgia yellow pine are the highest limits that the author would sanction for sound timber, he purposely left the unsound timber to individual judgment. Safe limits can be fixed approximately for sound timber, but who can assign a safe working stress to rotten timber? There have been cases coming under the author's notice where timbers were carrying the regular traffic up to the time of their removal, but when taken from the track and turned over on their sides they broke from their own weight, the interior being a mass of dry rot.

VI. The cited examples of overstrained bridges appear to indicate either that an all-live load does not produce twice the strain caused by an all-dead load, or else the usual method of figuring stresses by wheel-load moments is wrong. The author does not claim that a study

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of the examples cited upsets any of the well established theories of mechanics and statics. Is it a theory of mechanics that a load moving over a bridge is a suddenly applied load? Mr. Schneider has denied this to be a fact. Is it a theory of statics that the wheel-load moment method applied to a bridge is absolutely correct for the conditions existing? Mr. Schneider has shown that strains in small spans can be less than those figured by this method. He also shows that an examination into the first three cases reveals the fact that an added impact of from 60% to 65% would have strained these bridges up to the elastic limit, and since they were not strained to this limit the impacts were even less than given. The assumption that an all-live load does cause twice the stress produced by an all-dead load is made in all specifications which use the unit stress formula, $a = u\left(1 + \frac{\text{min.}}{\text{max.}}\right)$. If the elasticity be taken at 26 000 lbs. the value of u in the above formula must be not more than 13 000 lbs. in order that this limit may not be exceeded In applying the formula to the cases in question, it will be found that in every case the value of u exceeds the 13 000 lbs., and this indicates that the elastic limit had been passed; but as it really had not been exceeded, the author maintains that the examples tend to show that an all-live load does not cause twice the stress of an all-dead load, and that therefore the assumption is wrong, although it is wrong on the safe side.

VII. Engineers have sifted the good from the bad in bridge designs, and while there are differences of opinion as to what is and what is not just a little better than good enough, there is no question that all will agree upon what is fairly good construction, and, as was clearly set forth in the body of the paper, this class of bridge only is under discussion. The safety of a poor bridge cannot be discussed in general terms, but the safety of a good one can be. There are, of course, many cases in which the actual stress to which each individual member is subjected cuts no figure because the design may be poor and too shaky for the increased loads. There are also many cases in which it becomes necessary to limit the unit stresses in main members, although the structure apparently is capable of carrying still greater loads. Details should receive individual attention, independent of the attention bestowed upon main members, and in considering them it should be remembered that the strength of the weakest member in the weakest bridge on the line of a railroad is a measure of the capacity of the traffic on the road. If a single floor beam connection should break and cause a wreck, it would surely be poor consolation to know that all other connections had ample strength. Uniformity of strength is the desideratum of bridge construction. The quantity of strength is more or less a matter of judgment, and in proposing formulas for the maximum working stresses, which should be allowed in main members of

Mr. Greiner. bridges of good design, the author had in view uniformity of strength in main members, and the details strong enough to develop this strength. It will be noticed that some engineers, in considering the safety of an old structure, entirely disregard impact, and will allow a uniform safe working stress about 50% in excess of what they would allow on a new bridge where impacts are considered. This assumption if followed out will place suspenders and light members of trusses in positive danger long before the limits fixed have been reached in the main members. The author, however, understands that those proposing these limits use them merely as guides for final judgment.

In order to make a fair comparison of the limiting unit stresses which are considered safe, satisfactory or advisable by those who have ventured an opinion, the author has figured the flange and chord stresses in plate girders and trusses up to 200 ft. span. The limiting unit stresses given were then modified where necessary, so as to represent the quotient obtained by dividing the actual calculated stresses by the area of section of the member considered. The bridges were figured for 86-ton engines followed by 3 000 lbs. per foot, and a dead load given by Mr. Pegram's formula for this class of engine, with the addition of 350 lbs. per foot for weight of ties, rails, etc. The sections were determined by applying the formula used in the Baltimore and Ohio specifications.

Table A shows limiting stresses, the percentage of increase of the limiting stresses over the unit stresses used in the design, and the percentage of increase of live loads over the 86-ton engine.

An examination of the table reveals quite a variation in the limiting stresses for plate girders, but a much better agreement in those for the trusses. Had the table been extended, the proposed unit stresses would have increased, as would also those of Messrs. Wilson and Schneider, while those of Messrs. Pegram, Hazlehurst and Yeatman would remain constant. The author knows that Mr. Montfort would not adhere strictly to 15 000 lbs. as spans increased in length, and would consider the limits fixed by the author as approximating the same as he would probably use in such cases. Mr. Wilson has not fixed upon the units given opposite his name, but merely considers them safe, without saying whether he would go higher or not. Mr. Pegram's proposed units are very low for plate girders, although he stated in his discussion that he was inclined to agree with the limits fixed by the author. Messrs. Bates and Snow also seem to believe that the stresses proposed by the author are on the safe side when the conditions are as stated.

The limiting stresses for timber beams appear to be considered about right, although some would use 1 500 lbs. for white pine, and others would go as high as 2 000 lbs. for yellow pine, it being understood that the timber is perfectly sound.

TABLE A.—LIMITING TENSION STRESSES IN MAIN MEMBERS OF BRIDGES.

	Plate Girders.						Trusses.									
	30 ft.			60 ft.			90 ft.			150 ft.			200 ft.			
	Limiting stresses.	Percentage of stress increase.	Percentage of live load increase.	Limiting stresses.	Percentage of stress increase.	Percentage of live load increase.	Limiting stresses.	Percentage of stress increase.	Percentage of live load increase.	Limiting stresses.	Percentage of stress increase.	Percentage of live load increase.	Limiting stresses.	Percentage of stress increase.	Percentage of live load increase.
Proposed	14 040	75.5	88.2	14 690	72.0	92.3	14 880	69 6	93.2	15 560	58.1	84.5	15 950	56.5	88.1
Wilson	11 420	42.8	50.0	11 870	39.0	50.0	12 05 0	37.4	50.0	13 225	34.4	50.0	13 460	32.1	50.0
Pelton	10 840	35.4	41.5	12 050	41.1	52.7	13 140	49.8	66.7	15 000	52.4	76.2	15 000	47.2	73.6
Hazlehurst	13 500	68.7	81.6	13 500	58.1	74.5	13 500	53.9	72.1	15 000	52.4	76.2	15 000	47.2	73.6
Monfort	15 000	87.5	97.8	15 000	75.6	97.0	15 000	71.0	95.0	15 000	52.4	76.2	15 000	47.2	73.6
Featman	15 000	87.5	97.8	15 000	75.6	97.0	15 000	71.0	95.0	15 000	52.4	76.2	15 000	47.2	73.6
Schneider	11 510	43.9	51.7	12 860	50 6	64.8	13 400	52.8	70.5	14 430	46.6	67.8	15 120	48.4	75.5

Mr. Greiner. In closing this discussion, which has been carried to an extent not anticipated, the author desires to express his appreciation of the free and candid opinions advanced. The discussions have been of a practical nature, and the records of what bridges have stood and what they have not stood will surely be a future assistance to those who have questionable bridges to look after, and who are undecided as to what action should be taken. A good rule in case of serious doubt is to give the traveling public, not the bridge, the benefit. Iron is cheap when compared with human life.